MOTHER TONGUE

JOURNAL OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

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Mother Tongue Issue XII, 2007

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Introduction to *Mother Tongue* XII (2007)

The first section of this issue is dedicated to Harold C. ("Hal") Fleming, founder of ASLIP and *Mother Tongue*, who celebrated his eightieth birthday on the 23rd of December, 2006. We begin with greetings and memoirs submitted by some of Hal's many colleagues and friends, followed by two tribute articles by his friends Jean Lydall and Ivo Strecker. The section is closed by Hal's own "Autobiography of a Lucky Man." (We are indeed lucky to know him!)[1]

The Nostratic Hypothesis is one of the pillars of macro-family comparison and a major emphasis of the Moscow School (or Circle)[2] of comparative linguistics. The American scholar Allan Bomhard has also developed a version of the Nostratic hypothesis that agrees on many points with the Muscovite version (associated with Aharon Dolgopolsky and V.M. Illich-Svitych), while differing from it in some details of phonology. In this section Paul Sidwell and George Starostin respond to Bomhard's initial article, followed by Bomhard's response to the comments.

Ever since the pioneering work of Swadesh and Lees in the 1950s some linguists have sought to devise or refine a method of linguistic dating known as glottochronology. In this issue the Czech linguists Václav Blažek and Šárka Krpcová discuss the application of this technique to the Kartvelian language family, a universally accepted language family of the Caucasus region.

Human genetics, one of the branches of anthropology corollary to historical linguistics (along with paleoanthropology and archaeology), has always been a major focus of ASLIP and *Mother Tongue*. In this issue we offer contributions by Stanford geneticist Peter Underhill and the Hungarian team of Maria Agnes Solymosi, Edit Szucs, Katalin Barabas, and Matyas Mink.

From the beginning *Mother Tongue* has featured articles about so-called language "isolates," *e.g.*, Basque, Burushaski, Nihali, and Ainu. To most of us it is not probable that any human language is *absolutely* isolated from all others, but the *relative* isolation of some languages is intriguing. Here the British linguist Roger Blench gives us his take on two of them: Baŋgi me of Africa and Shom Pen of the Nicobar Islands.

We are pleased to offer in this issue another of Václav Blažek's studies of numeral words, this time on the numerals of the Dravidian languages of Greater India.

Finally, we feature two reviews of *The Horse, the Wheel, and Language,* by David W. Anthony, reviewed by the stalwarts of the Boston University Department of Anthropology, Daniel F. McCall and Harold C. Fleming.

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[1] A tribute volume (Festschrift) dedicated to Hal, tentatively titled *In Hot Pursuit of Language in Prehistory: Essays in Honor of Harold Crane Fleming*, is forthcoming from John Benjamins Publishing Co.

[2] This refers to the recent school (since the late 1960s) founded by Illich-Svitych, Dolgopolsky, and V.A. Dybo, not to be confused with the earlier Moscow Linguistic Circle that included Roman Jakobson and others.

Obituaries

Sadly we must report on the recent passing of two eminent members of the Moscow School (see previous page). Hal Fleming's meeting with the Moscow School in August of 1986 was the major stimulus leading to the newsletters that morphed into *Mother Tongue* (newsletter, and later journal), and eventually to the founding of ASLIP.

Grover Hudson has graciously sent us his remembrances of Marvin Lionel Bender.

Vladimir Orel

Vladimir Orel (Владимир Орел, born February 9, 1952) died in Calgary on August 5, 2007, following a massive stroke a few days earlier. Orel earned his PhD degree in 1981 from Moscow State University and began work the same year at the Russian Academy of Sciences, Moscow. Orel was the author of numerous publications, the most prominent of which were etymological dictionaries, including the *Hamito-Semitic Etymological Dictionary* (co-authored with Olga V. Stolbova, 1995), *Albanian Etymological Dictionary* (1998), and *Handbook of Germanic Etymology* (2003). After academic positions at Moscow University, Hebrew University, Tel-Aviv University, and Bar-Ilan University (among others), the last years of his life were spent in Calgary, Alberta, where he held teaching positions at Athabasca University, the University of Lethbridge, the University of Calgary, and Mount Royal College. At the time of his death Orel was completing his *Russian Etymological Dictionary*, which is now forthcoming from Octavia Press.

Eugen Helimski

Eugen Helimski (Евгений Хелимский, born March 15, 1950) died in Hamburg on December 25, 2007, of lung cancer. He earned a Dr. Sc. degree in 1988, was a member of the Russian Academy of Sciences (1978-1997) and Professor in the Russian State University for Humanities (1992-1998). Outside Russia he held positions at ELTE Budapest (1994-1995), the Free University of Berlin (1995), Humboldt University of Berlin, the Jagiellonen University in Cracow (1997-1998), and finally (1998-2007) Professor of Finno-Ugric and Uralic in the University of Hamburg and Director of the Institut für Finnougristik/Uralistik. Helimski was author, co-author, or editor of several books, for example, *Die Matorische Sprache: Wörterbuch – Grundzüge der Grammatik – Sprachgeschichte*. Szeged, 1997, 475 pp. [Studia Uralo-Altaica 41], and numerous articles. Helimski was a major authority in languages and cultures of northern Eurasia, particularly in Samoyedic languages (Enets, Nenets, Selkup, Mator) and Hungarian. For details see his homepage (http://helimski.com/) and his homepage at the University of Hamburg (http://www1.uni-hamburg.de/IFUU/personal/helimski.html).

Marvin Lionel Bender (1934-2008)

By Grover Hudson[3]

Marvin Lionel Bender, a prominent figure in Afroasiatic and Ethiopian linguistics for 50 years and whose works are among the authoritative sources on Omotic and Nilo-Saharan linguistics, died on Tuesday, February 19, 2008 in Cape Girardeau, Missouri.

Born August 18, 1934 in Mechanicsburg, Pennsylvania, he received Bachelor's and Master's degrees from Dartmouth College in mathematics, in 1956 and 1958, and Ph.D. in linguistics from the University of Texas at Austin, in 1968. His Ph.D. dissertation was a generative study of Amharic verb morphology.

Bender left PhD. studies at Yale to teach mathematics at Adisadel College in Cape Coast, Ghana. During a seminar on school mathematics at Entebbe, Uganda, he traveled to Ethiopia, and liked it enough to apply for and accept a position at Haile Selassie I University, where he became interested in Amharic and linguistics, and so returned to graduate school, at Austin, where his dissertation was directed by Emmon Bach. After Ph.D. studies Bender was immediately recruited to the research team of the Language Survey of Ethiopia, a Ford Foundation project (part of the five-nation Survey of Language Use and Language Teaching in East Africa), the other members of which were J. Donald Bowen, Robert L. Cooper, and Charles A. Ferguson. Bender was the only one with experience in Ethiopia and knowledge of Amharic, the Ethiopian *lingua franca*. The survey report, *Language in Ethiopia*, was published in 1976 (Oxford University Press), including several chapters by Bender, some co-authored with Ethiopian linguists. Words he wrote in the preface suggest the understanding about research conclusions which was to characterize his many books and articles in Ethiopian linguistics: 'an attempt to summarize the state of the art...and not a new source of orthodoxy'.

Over the years in the often contentious field of Ethiopian linguistics, in which different national and scholarly traditions compete, his freely expressed conclusions from research –especially concerning Omotic and Nilo-Saharan classification, in which his work became foundational– were frequently controversial, and just as often to be superseded by findings of his later work. He was among the first to take up the hypothesis of Harold Fleming about the status of Omotic as a separate branch of Afroasiatic, and that of Robert Hetzron about the internal classification of Ethiopian Semitic. Importantly, he succeeded in having both hypotheses accepted by the survey team and written into *Language in Ethiopia*.

When the survey was finished, Bender was appointed at Stanford University, to finish the Ethiopia Survey report, where he valued his continuing relationship with Ferguson and, newly, with Joseph Greenberg. In 1971 he joined the Department of Anthropology at Southern Illinois University (1971-2000), where he remained until retirement and for a time served as Department Chair.

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[3] Department of Linguistics & Germanic Slavic, Asian & African Languages, Michigan State University. Revised version of the obituary published in *Linguist List* 19.633 (Feb. 25, 2008).

His early research was to explore, with Ethiopia as an example, Ferguson's idea of 'language areas', and Greenberg's method of mass comparison as a basis for genetic language classification and a way to bring empirical process to bear in a little documented and diverse linguistic setting such as Ethiopia, with some 75 languages in four families. As Greenberg's classification of African languages had brought order and rationality to the broad field of African linguistics, Bender's would similarly serve Omotic and Nilo-Saharan. His early work also applied lexicostatistical methods to Ethiopian languages, work which his mathematics background prepared him for, but which sometimes enraged conservatives, who failed to distinguish lexicostatistics and glottochronology, or failed to see that his often original conclusions about Ethiopian-language relationships were more a test of the method, and working hypotheses, rather than attempts to establish 'a new source of orthodoxy'.

He was the first to systematically sort through the many problems of Ethiopian-language nomenclature, which had arisen from decades of research in four European languages and competing use of ethnic-group names, self-names, and Amharic names, and failure to distinguish dialect and language, and he was the first to attempt a catalog of all the languages and named dialects of Ethiopia, including a first attempt at a comprehensive genetic classification: *The Languages of Ethiopia* (*Anthropological linguistics* 13.5, 1971).

Turning to Omotic, Bender took to the field and began to fulfill the need for descriptions of many of these divergent Afroasiatic varieties. His thorough knowledge of prior work, ability to question informants in Amharic, and the new data he acquired enabled him to provide the first internal classification of this group, in his *Omotic: a New Afroasiatic Language Family* (1975), and eventually his *Comparative Morphology of the Omotic Languages* (2000), and *Omotic Lexicon and Phonology* (2003). He obtained grants, including from the National Science Foundation and Ford Foundation to study Omotic, and later Nilo-Saharan.

Soon he took up Nilo-Saharan, an extraordinarily diverse family, with often poorly accessible members. In order to provide the Ethiopian academic community, in and around Haile Selassie I University (now Addis Ababa University), with an affordable introduction to these largely ignored and often despised peoples and their languages, he self-published, in Addis Ababa, *The Ethiopian Nilo-Saharans* (1975). He was a Fulbright awardee at Khartoum University in 1978-9 and authored, with speakers of the languages, the only dictionaries on two of these languages: Gaam (1980) and Kunama (1996). He edited six volumes of Nilo-Saharan papers, and initiated the *Nilo-Saharan* (later - *Sahelian*) *Newsletter*. Bender's latest book on this family was *The Nilo-Saharan Languages: a Comparative Essay* (1996); he edited with Franz Rottland the Buske/Köppe series, over 23 volumes, *Nilo-Saharan Linguistics Analyses and Documentation*. He valued his time in the Sudan, but loved Ethiopia, where he returned many times. His Omotic and Nilo-Saharan works are a major contribution to the preservation of endangered languages.

Co-edited with Gábor Takács and David Appleyard, Bender's *Afrasian: Selected Comparative-Historical Afrasian Linguistic Studies in Memory of Igor M. Diakonoff* (2003), to which he contributed the 'Afrasian overview' and another article 'the Omotic lexicon', is probably now the best introductory source on Afroasiatic linguistics.

After retiring from Southern Illinois University, he found new time for his long interest in chess, and continued to write and publish with energy, despite failing health, recently completing a book on Cushitic phonological and lexical reconstruction, about which he was expected to present in March at the North American Conference on Afroasiatic Linguistics (NACAL), a meeting which he rarely missed and twice organized.

According to the obituary written for the Carbondale community, his ashes will be scattered in Baja California, Mexico, where he and his sons often vacationed in recent years, and perhaps an area which brought back for him memories of fieldwork in the west Ethiopian countryside. Memorials may be made to the Council for Secular Humanism, in Amherst, NY (www.secularhumanism.org/).

In Honor and Celebration of Harold C. Fleming

On the Occasion of his Eightieth Birthday
December 23, 2006

Greetings and Memoirs[1]

I express my best wishes to Professor Harold Fleming on his eightieth birthday. Fleming is one of the pioneer scholars who have a central place in Ethiopian Studies.

Between his fieldwork in remote and inaccessible parts of Ethiopia and his returns home, he supported staff and students of the Linguistics Department of Addis Ababa University by holding a series of seminars, giving computer training, and encouraging Ethiopian scholars to take part in his research. For example, in the first ever fieldwork trip to the now almost extinct Ongota speakers in the 1980s, he organized a research team of five scholars, among whom two were Ethiopians (Fleming 1992).[2]

Fleming is a humane and concerned researcher who sought help for the people in his research area at critical moments. During his fieldwork on their language in the 1970s, the Dime were attacked by their neighbors, the Bodi (linguistically members of the Nilo-Saharan family). After several hazardous trips to the regional and main capitals (Arba Minch and Addis Ababa respectively), Fleming successfully pleaded with the authorities to intervene and stop the violence (cf. Fleming 1994).[3]

He also has a deep knowledge about the socio-political climate and history of Ethiopia, and in the supposedly 'neutral' field of study, linguistics, he was known for referring to some of the country's problems with an appropriate sense of humor. Some (non-linguist) colleagues remember to tell a paper he gave in Lund, Sweden in 1982. At the time when Maoism suffered global decline and disrepute, Fleming, in a well-attended session at the International Ethiopian Studies Conference came up with a paper entitled: "The importance of Mao in Ethiopian history" (Fleming 1984).[4] Those political scientists and historians who eagerly flocked to his lecture were disappointed when it appeared that his paper was on the Mao (Anfillo) language of south-west Ethiopia. Similarly, in 1997 when the politicization of language-based ethnicity in Ethiopia reached an absurd level, Fleming gave a paper to a brimful large conference hall in Kyoto where not only scholars of Ethiopian Studies but also representatives of various regional administration authorities were present, who were treated to a pseudo-genetic critique of linguistic/ethnic group difference.

---

[1] In alphabetical order by author's surname.

[2] Fleming, Harold C., Aklilu Yilma, Ayyalew Mitiku, Richard Hayward, Yukio Miyawaki, Pavel Mikeš, J. Michael Seelig. "Ongota (or) Birale: A Moribund Language of Gemu-Gofa (Ethiopia)." *Journal of Afroasiatic Languages* 3, No. 3: 181-225.

[3] "The Dime of Gemu-Gofa: ethnography of a tragedy." In: C. Lepage, *et al.*, eds. *Études Éthiopiennes. Actes de la Xe Conférence Internationale des Études Éthiopiennes, Paris, 24-28 Aôut 1988.* Paris: Société Française des Études Éthiopiennes, vol.1, pp. 449-451.

[4] "The Importance of Mao in Ethiopian History." In Sven Rubenson, ed., *Proceedings of the Seventh International Conference of Ethiopian Studies*, Lund (Sweden), April 25-29, 1982. 31-38.

Fleming was courageous to come forward with controversial but sharp observations also in linguistic description and classification. This is the case with his "Omotic hypothesis," which brought him much fame as well as criticism. According to this hypothesis, Fleming split off a large number of languages which were originally classified as members the Cushitic language family and re-classified them as a new branch of Afroasiatic, which he labeled the *Omotic* language family. Fleming's classification gained acceptance by a number of scholars in the field, like Lionel Bender and Richard Hayward who furthered research into the languages of this family. The Omotic Hypothesis is still debated. However, as Hayward (1990: viii)[5] writes, the separation of these languages from Cushitic "has proved a methodologically sound move as far as satisfactory reconstruction is concerned." Still, more research on different aspects of these languages is needed for conclusive classification; and most of such work will depend on Fleming's studies on the lexicon and grammar of least-known minority languages.

Dr. Azeb Amha
University of Leiden

* * * * * * *

Dear Hal, May you have a happy birthday, and many more to come. For sheer imagination and boldness of vision you certainly have few equals.

Robert (Bob) Blust
University of Hawaii

* * * * * * *

Many thanks for your kind offer to contribute to the *Festschrift* for Professor Fleming. I feel very much honored that Hal invited me. I know him from a lot of nice correspondence as a very engaged person and I feel a bit sorry that I could not do much in the past for *Mother Tongue*. The more I regret that it will be impossible for me to write an article for the *Festschrift*. Please pass on my cordial greetings to Hal and my sincere thanks for thinking of me for his important *Festschrift*. I very much hope he will forgive me and will understand my situation.
Best wishes,

Günter Bräuer
Universität Hamburg

* * * * * * *

Dear Hal, Congratulations to a true Long Ranger, in both senses of the term!

Merlin Donald
Case Western Reserve University

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[5] Hayward, Richard. 1990. Introduction. In: Hayward, R.J. (ed.), *Omotic Language Studies*, 425-493. London: School of Oriental and African Studies.

* * * * * * *

Harold has been for me one of the most stimulating colleagues in African linguistics. I wished we had more like him in our field.
With best wishes,

Bernd Heine
Netherlands Institute for Advanced Study

* * * * * * *

I would like to thank Harold Fleming for inviting me to participate in his Festschrift. Kudos to you Harold for a life well spent in the pursuit of knowledge to further a positive understanding of the African past.

Shomarka O.Y. Keita
Howard University,
Smithsonian Institution

* * * * * * *

Back in the fall of '68 I went off to graduate school. There were five professors of Anthropology at Boston University, with the widest possible range of personalities—I still see three of them on occasion, and perhaps most often, the one with the red hair, blue eyes, and "Ain't the world fascinating" grin. I think I had only one class with him—three students and Hal, delving into the delights of Bantu linguistics. Hal was always 15 minutes late to class. That was a fact of life that all students knew and accepted, and did so without complaint in those days. Of course, he would then stay half an hour after class, making one wonder how he would make his next appointment.

There was a department party one evening . . . I came in a bit late, and there were all the sociologists, properly sitting and sipping in the living room, with emphasis on the proper . . . I wandered back into the kitchen, and there was Hal, perched on the counter, beer in hand, grinning as always, rapt in the enthusiasm of his anthropology and his linguistics . . . .

It didn't matter that I had few classes with him—he was still a central part of my graduate school experience . . . I remember sitting in his office, pouring out all the latest on Bantu expansion and the Iron Age, while he sat there, grinning in utter fascination, as if I were revealing the mysteries of the universe to him—on a subject he no doubt knew far better than I. I got my M.A., and went across the river for the Ph.D., but kept teaching at B.U., so he was a full part of my whole graduate career.

I wandered off to Africa and other places, but when I returned to Boston, my wife took up her undergraduate studies at B.U.—in anthropology. Hal said to her, "Now, Lisa, there are these sources on Ethiopia, this one's in German, the multi-volume one is in Italian . . . you think you might do papers on them . . . ?" At about page 100 of her rendition of the Italian one, I, who was doing the typing for her (on an old, old manual), said "Let Hal type his own damn translations . . ."—the man could really inspire work from his students.

I have always been an admirer of Jimmy Carter, and of his presidency. So, too, of Hal and his professorial career—but both Jimmy and Hal are also celebrated for what they did afterwards. It may not have gotten him the Nobel Prize yet, but through the founding of *Mother Tongue* and his boundless enthusiasm and unrelenting energy, Hal has accomplished just about as much to make this world a wiser and better place.

I have family in Gloucester, where Hal resides. Many years ago, while visiting Cape Ann, he did me the great favor of introducing me to Sam Adams beer, back before everybody knew about it. And, on a recent visit, he came by the house . . . Willa, my three year old granddaughter, saw him, and without hesitation broke out into the same contagious, engaging grin . . . even the little children suffer Hal to come unto them . . . .

Larry Lepionka
College of Charleston, South Carolina

* * * * * * *

It was 1959-60, and we shared a duplex in Casa Incis, and we held a Seminar on Ethiopian Studies at the University College of Addis Ababa where the likes of Sven Rubenson, Zewde Gabre Selassie, Richard Pankhurst, William Shack, were breathless with their fresh discoveries. It seemed like Ethiopian Studies in a new key was truly getting off the ground.

And then came Herb Lewis and Hal Fleming, really breathless, back from extraordinary travels in Gemu Gofa where they had discovered ethnies and tongues never before recorded, and leading Hal to propose the radical idea of reclassifying West Cushitic language group, not as a just another Cushitic cousin but as a family in its own right.

Bliss it was in that time to be alive and well in Ethiopia, and to be young and privileged to penetrate fresh scholarly territory was a treat beyond words. Hal, I shall never forget those precious times.

Donald Levine
University of Chicago

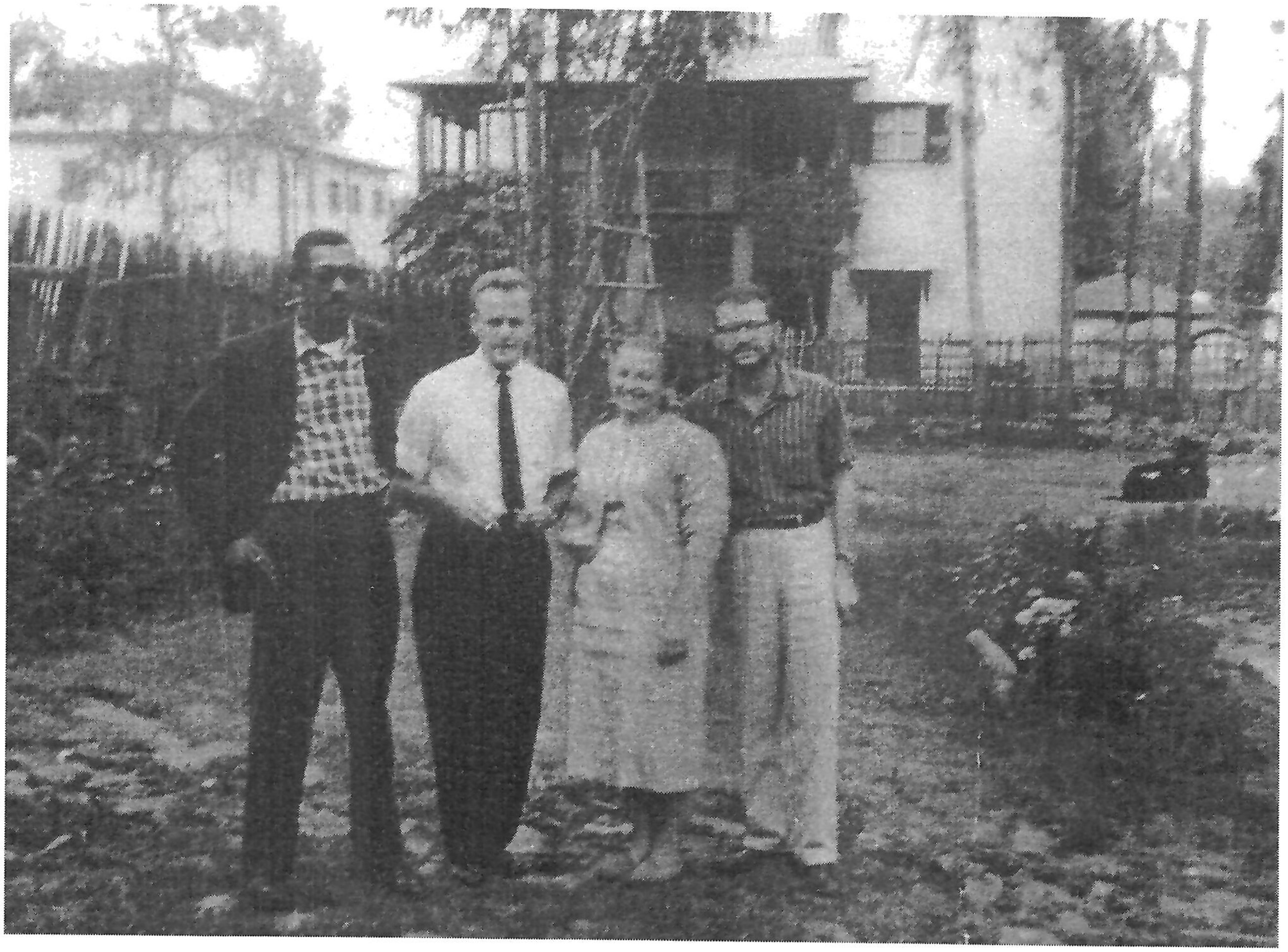
* * * * * * *

On January 11, 1959, my wife Marcia, Richard Kluckhohn, and I, drove from Addis Ababa south to the town of Sheshemane. As we entered the restaurant of the town's small hotel, Dick said, "That's Hal Fleming over there," and a friendship of almost fifty years was born. That night and the next Hal and I talked until early in the morning about linguistic, physical, ethnographic, and archeological evidence for culture history in Ethiopia and Northeast Africa. Before we left the US for Ethiopia and our separate research agendas, Hal and I had each asked Joe Greenberg what we might do to be helpful for the cause during our research. He directed us to the need for much more information about the languages of southernmost Ethiopia, especially those of the Bako group and Gemu-Gofa generally. We decided that night to make a joint "expedition" to that area to collect languages.

A few weeks later, Hal, Marcia, and I, with Hal's teenage hired hand, Fakir, made an often difficult, sometimes frightening, but always exciting and memorable five-week trip. We negotiated long-abandoned roads, eroding away since the Italians were driven out 15 years earlier, with our little jeep and trailer. (We couldn't have done it at all had not Hal known someone who could arrange to have the air force fly a half drum of gasoline down to Bako in its fuel tanks.) We had adventures with people, wildlife, malaria, and came back with wordlists for almost 20 languages, increasing the basic information on some of them more than 10-fold. And Hal was able to add to new languages to the record from that trip: Woraze and Gobeze.

Fifteen months later, in May 1960, Hal, Marcia and I set out again, in a beat-up old Peugeot van, traveling from Nairobi with a plan to see East Africa and then cross the continent to

Addis Ababa, 1959



American anthropologists William Shack, Harold Fleming, Marcia Lewis, and Herb Lewis.

Photo courtesy of Donald Levine.

Nigeria. After traveling through much of Kenya, northern Tanganyika, and Uganda, we only got as far as Stanleyville (Kisangani) in the Belgian Congo when we were forced by political unrest (notably by very bad experiences with nasty soon-to-be ex-colonials) to turn back to Ruanda and Uganda. We had another five weeks of great adventure, and collected several more wordlists as well, from northern Kenya and northwest Tanganyika.

Throughout these months of travel, and in many hours spent talking in various homes, parties, and bars in Addis Ababa, and at any time that we have met subsequently throughout the years, anywhere in the world, Hal's passion for, and his tireless devotion to, historical linguistics and the study of the long-range development of human populations and cultures has been front and center. Hal Fleming has devoted his life to this cause, clearly taking delight in every aspect of it. He was an adventurous and tireless worker in the field, in remote areas difficult to get to, and his efforts paid off in the collection of much material on previously unknown languages. His discovery and recognition of the place of Baiso, for example, was a powerful contribution to our understanding of the origins of Eastern Cushitic and of the Oromo and Somali peoples.

I must leave it to others more qualified than I to speak of his many analytical achievements, but his insight into the classification of the Omotic languages was surely a major contribution. And I must mention the great good will, humor, kindness, and generosity that have always characterized his work and his relationships.

In addition to having learned a great deal from Hal (including how to double clutch and handle a four-wheel drive vehicle over some of the world's worst roads), Marcia and I have always had great fun with Hal. We are happy and proud to have had this most decent and wonderful man for a dear friend all these years.

Herbert S. Lewis
Professor Emeritus—Anthropology
University of Wisconsin-Madison

* * * * * * *

Happy birthday, and warmest of wishes Hal!

There is no better opportunity than this for me to give you my sincerest thanks for the involvement and influence that you have had on my development as a historical linguist. It was more than ten years ago in 1995, living in Taiwan at the time, when I began to develop a serious interest in historical linguistics and long-range comparison; I didn't know the field at all at that time, but I collected contact information from people as best I could. I wrote asking for advice as to how to proceed with my education if I wanted to pursue this seriously. It was Merritt Ruhlen who first advised me to contact you and John Bengtson, and I received very kind replies from both of you and my first official encouragement to pursue historical linguistics as a full-time interest.

Of course, along with this encouragement came the offer to join the Association for the Study of Language in Prehistory (ASLIP), which I did with great relish, and by the time I had returned to the United States to begin graduate school, I had received a copy of the very first journal. Not long thereafter, you asked for help shouldering the enormous workload you had been undertaking in order to maintain ASLIP and its publications; it was at this point that I had the privilege of taking the office of Treasurer and then Secretary for several years, before eventually relinquishing them to enter the field for dissertation fieldwork. The behind-the-scenes work, for which there was no paycheck, can be more formidable than people realize, and it is also easily forgotten. Thanks to this experience, I am able to appreciate all of your hard work over the years and empathize with you during the times when it must have felt overwhelming, as you not only did the job that I took for a time, but acted as editor during long stretches of time as well.

Of the few times I have been able to meet you face to face, my best memory is of the time when you traveled to Tucson in 1999 to stay with my family in order to see a talk given at the University of Arizona by Joseph Greenberg. To any long-ranger, no matter what their personal methodological inclinations, Greenberg is an icon, having been one of the foremost pioneers in long-range comparison. As we all know, Greenberg passed away not long after that talk, and it was my only opportunity to see him speak in person. The event was greatly enhanced for me because you were in the audience as well, and I knew that there was a like mind with which to share that exciting experience.

Hal, I admire your endurance, tenacity, and keen sense of humor in the face of trials and adversity, which you are certainly no stranger to in your academic career. Congratulations on all of your accomplishments to date, and I wish you the happiest of birthdays, and hope you enjoy this present. Sincerely,

Peter Norquest
University of Arizona

* * * * * * *

Dear Harold, Best Wishes on your Birthday! You continue to be an inspiration for those of us interested in the genetic and linguistic history of Africa. I look forward to many more energetic discussions with you on the topic. Best Regards,

Sarah Tishkoff
University of Maryland

* * * * * * *

Encouraging collaboration and cross-fertilisation between Soviet and non-Soviet linguists at a time when there was an iron curtain to discourage that sort of thing.

Establishing a forum (Mother Tongue) for the discussion of paleolinguistics and its integration into the wider study of human prehistory; keeping linguists *au fait* with the latest in archeology and biogenetics, and vice versa.

Going out into the field and coming back with languages like Shabo and Ongota.

Establishing Omotic in its true place within Afrasian.

Consistently leavening the mumbo-jumbo with common sense.

Promoting paleolinguistics without ever being sectarian about it; never failing to welcome the contributions of non-paleolinguists.

Encouraging and enthusing wannabe linguists like me, for whom he is an exemplar and guide.

Paul Whitehouse
Santa Fe Institute

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Pronunciation Puzzles

Jean Lydall
South Omo Research Center,
Jinka, Ethiopia

Dear Hal, do you remember how, way back in 1971, Ivo [Strecker] and I used to drop by your flat at Arat Kilo in Addis Ababa! And do you remember the following story that Nancy once told us when offering peanuts to accompany our beer? One of the twins, who must have been about three years old at the time, had come to Nancy in great distress, crying "Mami, where's my pinas!" Nancy marvelling at this Freudian revelation tried to console your distraught daughter by saying, "Sweetheart, you don't have a penis, you are a girl," upon which she declared angrily, "I do too! I had a whole jar full of 'em."

As you know, Ivo and I had come to Addis Ababa with our two small children, Theo (2½ years) and Kaira (7 months), and our great Hamar friend Aike Berinas (also known as Baldambe), because I had fallen ill with hepatitis, and needed to recuperate. Since March 1970, Ivo and I had been doing anthropological research in Hamar, Gamo Goffa, with a six month interlude for me in England to give birth to our daughter Kaira. We had attained a good working knowledge of Hamar language, and Ivo had started recording local accounts of Hamar custom with the idea of turning them into a book. Then suddenly we had to leave Hamar because of my health. Ivo was extremely unhappy at this rude interruption, and I wasn't much happier given my jaundiced condition. So it was a great relief when Ivo came home one day with the cheering news that he had met some really interesting people, the Flemings. At the first opportunity we met up with you and your family, that's to say, your wife Nancy with twin girls and infant son, and your eighteen-year-old daughter, Leslie. We immediately became the best of friends, sharing as we did similar enthusiasms for the diversity of peoples, languages and cultures in southern Ethiopia, as well as good food, drink and humour.

Hal, you told us how you had spent many years researching the genetic affiliation of Ethiopian languages into language families and sub-families. Your hunch was that Hamar belonged to a sub-family of Afroasiatic that was formerly called West-Cushitic, but should now be called by another name, perhaps Omotic, because it clearly derived from a different ancestor to the rest of Cushitic. You yourself were doing research into Dime, which you said was closely related to Hamar and Ari, and together with them constituted the weakest member of this sub-family. Neither Ivo nor I were linguists, but you were pretty impressed with our command of the Hamar language when hearing us translate the stories and views of our friend Baldambe, and so you encouraged us to work on lexical and syntactical aspects of Hamar. It was through you that Lionel Bender got in touch and invited us to contribute to his volume on the non-Semitic languages of Ethiopia.

In the grammatical sketch of Hamar language that I wrote for this volume, I dealt with Hamar phonology at some length, exploring categories, junctures and lengths of vowels, as well as categories, changes and clusters of consonants. I also looked at stress and pitch, but as regards tone I was baffled, and declared defensively, "I have not yet discovered the role of tone in Hamer." (1976: 405) Having looked anew at what I wrote so many years ago I am at first impressed at what I had discovered in my youth when my ears could still hear and my eyes still see. But I also notice how, in the arrogance of youth, I had presented the phonology as if I had solved all pronunciation puzzles, besides perhaps the question of tone. Now, thirty years later and humbled by age I want to consider some of the pronunciations that still puzzle me, and to ask you whether you could help solve them.

There were a couple of things that I was unhappy with when my grammatical sketch went to print. The first is the consonant depicted there as an ejective *k'* because Bender insisted that *k* could not be implosive as I was thought it could. Years later, Hal, you were able to reassure me that an implosive *k›* is indeed a possibility. In recent years I have often noticed that Amharic speaking Ethiopians working in Hamar use an ejective *k'* instead of an implosive *k›* when speaking Hamar. Is this because they are only familiar with ejective *k'* and therefore both hear and reproduce it as such, or is it because it is actually an ejective *k*' and I have been hearing it incorrectly all these years? Fortunately, when I asked Awoke, the school educated son of our friend Baldambe, he confirmed that the *k* in question is indeed implosive, and as there is no character in the Amharic script to depict it, Hamar speakers, and speakers of other languages that use implosive *k›*, have adapted an Amharic character to depict it, as they have for implosive b, d and g. Although I often think I can hear the difference between explosive, ejective and implosive *k*, I can rarely pronounce either the Amharic ejective or the Hamar implosive correctly. Unlike Nancy's daughter who was still young enough to learn how to make a clear distinction between penis and peanuts, I was already too old at the age of 25 to learn to pronounce certain Hamar sounds without a glitch.

The second thing I was not too happy with was that Bender was adamant that American English speakers would mispronounce the name of the language if it was written Hamar, and that is why it should be written Hamer. I don't know if this is true or not, or whether any American English speakers ever get to pronounce the name at all, but because of his insistence linguists like Siegbert Uhlig persist on rendering the name incorrectly, even when supposedly using phonetic script. For example, my entry on Hamar language in *Encyclopaedia Aethiopica* was changed by the editors to Hamär, although I told them this was neither phonetically right nor the way Ivo and I had been writing the name for the past 30 years. (Lydall 2005: 983).

Not everyone agreed with the analysis of Hamar phonology that I had made. One Ethiopian student of linguistics claimed there were only five vowels, rather than the ten (five category I and five category II vowels) that I had postulated. This baffled me; did we hear things so differently, and if so was this because we had different mother tongues, or was the difference I heard between category I and II vowels to be explained by tone, the thing I had yet to discover, or by other factors? Could it be indeed that there are only five vowels, but in certain environments their sound changes? For example, after an implosive consonant or glottal stop, or within a syllable ending in a consonant, maybe a vowel becomes more open, hard, squeezed, creaky, unraised, unmarked, and pronounced with constricted pharynx, whereas otherwise may be it becomes more close, breathy, hollow, raised, marked and pronounced with open pharynx. Hence the distinction between *ami* meaning 'field' and *ami* meaning 'breast' would be explained by a slight glottal stop in front of the term for breast causing the following *a* to be pronounced with an open pharynx and advanced tongue root position. Likewise a slight glottal stop would have to be assumed to explain the difference between *ono* meaning 'house', and *?ono* meaning 'female calf'; *ela* meaning 'call forth', and *?ela* meaning 'seep'. Syllable breaks, on the other hand, might explain the phonetic difference between *pi'ta* meaning 'shit (small particular form)', and *pit'a* meaning 'second growth of sorghum'; *wu'ta* meaning 'sharpen', and *wut'a* meaning 'impure conception'.

In 1986, Baldambe visited us in Germany for the second time. In my notebook I made the following entry written 28.8.1986:

> Baldambe points out the difference between the pronunciations of *inta* (pronoun for 1st. person singular) for male and female speakers. We have lived in Hamar so long and never realized this difference. Even now I can hardly hear it. I think the male speaker says *ìntá*, the second syllable having a higher tone than the first; the female speaker has same tone, low or mid, for both syllables.
>
> *intá ká* (I that one m.) cf. *inta kò'rò* (I that one f.)
>
> *intá sesáé* (I who did wrong m.) cf. *inta sesònò* (I who did wrong f.)
>
> A girl who says *intá ne* – it's I, will be corrected, *ya angu? intá an amaino?* - are you male that you say Í?
>
> To distinguish between 'you male' and 'you female' one says *ya ká* and *ya kòrò*.
>
> Tone!
>
> We were guests of Wolf Dietrich the other evening and he and one of his other guests suggested there was tone difference between *duka* – plant (verb), and *duka* – mountain. The former seems to

> be low, low, the latter high, low. Also the words *boaka* – meeting ground, and *boaka* – scoop (verb); the former has mid-tone on both syllables, the latter has low lone on the first syllable.

As I write these lines, Duka, daughter of Baldambe, is staying with us as special guest in our German home. I checked with her once again the pronunciation of these various words. As regards the gender difference in the pronunciation of *inta*, Duka tells me the same as her father before her; that a man may stress the second syllable, but a woman should not, and instead, she should use/put the same tone/stress on both syllables.

Like her father, Duka insists that the pronunciation of *boaka* is different depending on whether one means 'meeting ground' or 'scoop' (verb). I note that the tone or stress is equal for the meeting ground, and low on the first syllable of scoop. Duka points out that for meeting ground the air goes out, and for scoop it goes in. Does this mean the main difference lies in the consonants, that meeting ground uses an explosive *b*, and scoop an implosive *b›*? And how about the *k*, is it explosive in the one word and implosive in the other? The two syllables for meeting ground seem to be of equal length, for scoop the first syllable seems long. Is length of syllable the main difference? Could the difference in tone/stress and syllable length be due to the one word being a noun and the other a verb? Or are they due to the use of explosive *b* and/or k in the one case and implosive *b›* and/or *k›* in the other? In order to pronounce the words to Duka's satisfaction, I try to think of the word as having two syllables of equal length, stress and pitch which I utter breathing outwards, *'boa'ka,* to mean 'meeting ground', or of unequal length, stress and pitch which I utter breathing inwards, *'b›òà:k›á,* to mean 'scoop'.

Even Duka's own name raises pronunciation puzzles. Her name means 'plant' (verb) and, she tells me, is pronounced quite differently from *duka* that means 'mountain', which in turn is distinct from *duka* that means 'bury'. Where does the difference lie between these words? One difference may be the consonants. The *d* and *k* in Duka's name seem to me to be implosive, but for the other two words they are explosive. Another difference could be tone, with Duka's name having low tone on the first syllable, *d›ù'k›a* 'mountain' having high tone on two syllables, *dú'ká,* and 'bury' having mid tone on both syllables, *duk'a*. A further difference could be the syllable breaks as indicated by an apostrophe. Straining to hear, and watching Duka's mouth closely, I struggle to identify the differences between these three words. But, whatever answers I imagine, how do I know which are correct? Ivo says there is a computer programme that can graphically depict sounds, and suggests that if we had it we could surely solve all pronunciation puzzles. Is that true? Whatever the case, by far the best would be if we could meet again, Hal, and over a glass of beer and a jar of peanuts, we could discuss and debate such like puzzles and their multifarious solutions.

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"Rhetoric Culture Theory"
Account of a title search

Ivo Strecker
Johannes-Gutenberg University, Mainz;

*

Letter to Hal

In my memories – some of long ago, some more recent – I always see you bright eyed, moving speedily (in Addis Ababa, in the field, at Ethiopian and international conferences) with a broad smile on your face. Where are you storming, and why this partly inward, partly outward smile? Obviously you are animated. Something amuses you. Is it that you laugh in anticipation of some further academic outrage that you are about to cause?

I have always admired you for the fearlessness with which you move forward both in empirical research and theory building, knowing quite well how more sober colleagues will scold you, reprimand you and dismiss your daring ideas as childish. But you cultivate playful, ironic naivety in order to be free, think freely, conjure possibilities that leave familiar ground and go far beyond anyone's imagination. Today, as 'grand narratives' have been abolished in the social science, and as it has almost become taboo to aspire to anything large, you have chosen the role of the trickster, and this, as I see it, is the key to your scholarly success.

In a way, your example gave me the courage to muster the support of Stephen Tyler and other colleagues in the fields of linguistics, rhetoric and anthropology to start something that is similarly 'naïve' and ambitious as your *Mother Tongue* project. To give you an idea of what this venture is about, and how in recent years I have cultivated my own brand of academic 'naivety', I provide you here with an account of a title search.

The search started in the early spring of 2003 when several contributors to the international *Rhetoric Culture Project* began to engage via e-mail in an unexpectedly long and complex debate about an appropriate title for the first volume in the new Berghahn Books series *Studies in Rhetoric and Culture.* Originally, the title of the book was to be *Rhetoric Culture Theory* because this had been the title of the conference to which contributors were invited (see www.rhetoricculture.org). But when Marion Berghahn took our series on board, she suggested that the books should not be too voluminous and that it would be better to split Volume I into two. I suggested that the title for the first volume should be *Rhetoric Culture Theory* and the second *The Constitutive Interplay of Rhetoric and Culture*. For a while no one objected, but then Felix Girke – one of my doctoral students – said that he wanted to veto *Rhetoric Culture Theory* because this title meant that the first volume would usurp a leading role in the series. So we began to search for another, perhaps more appropriate title, but as it turned out, Felix - who soon was off for fieldwork in southern Ethiopia – had left us with a problem that was impossible to solve. Here now is the story of what happened. In order to retain the course of our debates I present the different steps of argumentation one by one, and to facilitate reading I give each step a short title. Hoping that you enjoy this story, and calling you, as in our Ethiopian days, 'hunting friend' (*misso*), yours always,

Ivo

*

Veto against 'theory'

As I have said above, the first step was a veto against the title *Rhetoric Culture Theory* because some of us found the term 'theory' repellent, saying it resonated too strongly with imperial, even godlike concepts such as 'spirit', 'universality', 'transcendence' and so on. So if 'theory' was suspect, and unacceptable what else should we use?

Half-hearted 'exploration'

Felix, Christian and I first came up (half-heartedly) with *Exploring Rhetoric Culture* and then informed contributors about it. Did anyone object? The first one to answer was Todd Oakley who was not really happy with *Exploring Rhetoric Culture* but did not say so directly. Rather, he praised a second title (*The Interplay of Rhetoric and Culture*) saying it was "particularly apt" and said nothing about the first.

Then Jon Abbink wrote: "The working titles of the volumes look fine to me; although the first is a bit too short and dry. As it is something new that you offer in the series, the first title should be something like *Rhetoric Culture: Exploring the Dynamics of Language, Discourse and Society*, or *Rhetoric Culture: Discourse and Meaning in Social Interaction*, or *Rhetoric Culture: the New Paradigm* (ambitious, echoing E.O. Wilson*)*.

The problem with 'paradigm'

We immediately found Jon's suggestion very attractive. He had done several things: Firstly he had divided our title into two, a basic title and a subtitle. Secondly he had stressed that it was something new. Thirdly he had brought out in the open what Christian, Felix and I were secretly hoping, i.e. that the rhetoric culture project was to launch a new paradigm for anthropology and cultural studies in general. Forgetting that certain hopes and dreams better remain unsaid, we immediately sent out another circular proposing that the first title should be more daring for example like *Rhetoric Culture: the New Paradigm* or *Rhetoric Culture. Towards a new paradigm*.

Without realizing it, we were back to the godlike position that we had rejected in the first place, only that 'paradigm' was pretending to be less metaphysical than 'theory', more oriented towards practice, and, as we soon realized, oriented towards engineering, manipulation and subservience to the natural sciences. Jon had noted that our title was a bit weak and did not go well with 'rhetoric culture', a collocation of terms that is new and implies provocation and an ambition to be at the 'cutting edge' of the discipline. But, like us, Jon was not quite aware of the fact that at least in the USA 'new paradigm' has become "a bit of cliché".

It was Ralph Cintron who first pointed this out when he wrote: "Concerning the book title, however, I am a little suspicious, personally, of any title with the term "new paradigm." In a curious way, this is the sort of thing that I critique in my essay. Moreover, the very idea of rhetoric culture, your own observations regarding Humboldt, etc.--isn't it part of the point to talk about a long history of this perspective? Certainly, this is a different inflection but the rhetoric of 'new paradigms' seems frankly not so new, even a bit of cliché."

The problem with 'new'

As Ralph noted, not only 'paradigm' was a problem but even more so the idea of the 'new'. 'New' opened a vast echo chamber of debates on modernity and post-modern culture like for example the following lines in Stephen Tyler's paper on 'Meditation on Mediation':

> "Invention, in the classical tradition, was not the discovery of the new, but was literally the "coming-in (*in-venere*)of what was already known. Creativity, as the recombination of the known, was guided and facilitated by tradition. Tradition was thus not the burden of the past, it was the past as the means of the present and future, the concrete image of the co-implication of the moments of time. These ideas contrast strongly with modernism where invention is understood as the discovery of the new and is regarded as the engine of enlightenment that overcomes the obstacles of tradition, and time is only the present/future. All the idolatry of science and modernism is captured in this contrast. The *production* of new knowledge is the *industrial* metaphor of modernism. This topic is too complex for this short paper, but what I am suggesting is that this production metaphor and all the baggage associated with it is suspect in an age where exchange is the central theme and the circulation and re-circulation of recombinant virtual products is the dominant metaphor.

> Rhetoric/Culture seeks to understand this emergent return to the trans-cultural context of classical rhetoric. It is a kind of detour through the past in which the past is not just something to be discredited or overcome in the production of the new, but it will have been instead the means of a kind of appropriation of the future already present" (Tyler 2002:11).

I wrote to Ralph and thanked him for putting us back on course, saying: "I share your assessment of 'new paradigm'. Very good that you stop us from going the wrong direction! The title of the books has to be in tune with what Stephen Tyler - and others like you and me - have been saying and writing. The great side of our project is that it does not negate the past but acknowledges the constant flow and the emergence and disappearance of theories and their social and cultural circumstances. 'New' was the fetish of modernity, and in your contribution to the conference on rhetoric culture theory you, Ralph, have addressed the negative even 'dystopic' echoes of 'new' very well saying:

> "As so many have argued, the promised utopia of modernity has devolved into dystopia, and the vision of an enlightened, rationalized social order has become global disorder. If these are the conditions that our times have hurtled into (and not all would agree, particularly the keepers of wealth and power who profit from modernity), it is understandable that we would convert events into signs and read the experiences of the last century – the world wars, the various forms of holocausts and ethnic cleansings, the large scale environmental stresses, the endless threats of different kinds of mass destruction, the vast and unequal distribution of global capital – as brilliant spectacles of a bankrupt modernity playing itself out with increasing rapidity on the largest stage possible, the entire planet" (Cintron 2002: 3).

Anthropology of the Said and the Unsaid

Ralph had made us change our direction, but he had not ventured to formulate an alternative title. So I kept pondering and discussing the matter with Christian and then wrote another circular proposing "Rhetoric Culture. Anthropology of the Said and the Unsaid", a title on Stephen Tyler's most seminal work, "The Said and the Unsaid". I closed the letter adding: "As you can see, I am eager to find a title that all of us like and fully agree with. Could this be IT? Please let me know."

At first there came answers that mostly signaled agreement.

Brigitte Nerlich, "I love it!"
Jean Nienkamp, "Very nice. I like it a lot. It fits with my idea of cultivated and primary internal rhetorics, too";
Ralph Cintron, "A bit elusive, which is probably good. Probably the best title yet. I hate to say it though – I still don't quite know what "rhetoric culture" is & so I think we have yet to name the core issue;
Pierre Maranda, "It sounds very good, Ivo. However is not «Theory» dropped in that title, which is an important component of RCT? Of course it may be said that theory is recuperated by «Anthropology of». Yet, I so much like RCT...";
James Fernandez, "Of course a title for the first volume reflecting Stephen's work would be a good thing";
Francois Douay; "This is certainly the best you suggested ("anthropology" is more precious –and exact- than the already worn out "paradigm"...) being an advocate of complexity and graduality, I am never enthusiastic with positive/negative oppositions, such as said/unsaid: so much is "half-said/ half-unsaid", or just hinted at, or "said but not meant", and so on... but, of course, a title has to be short and striking; so : OK for "*CULTURE RHETORIQUE. ANTHROPOLOGIE DU DIT ET DU NON-DIT*" but why not "*RHETORIC CULTURES*"?: "Cultures rhétoriques. Anthropologie du dit et du non-dit" ? we are really plural, and, to me, it's a great quality";
Brigitte Nerlich, "I am tempted to say: a very trying title indeed!!"
Michael Carrithers; "A super title, one I'd be proud to have contributed to."

The illusion of having arrived at a consensus lasted only a few days, that is until James Fernandez had thought things over and sent us the following letter: "I must say that I am not very enthused with the overall title "The Said and the Unsaid". Not only does it repeat unimaginatively and too obligingly a good title for Steve's original work but it does not quite suggest the dynamic theory that I and others – and I think Steve – over the years, have been putting forth ... let me suggest "The Spoken and the Unspeakable" as a title truly original and interesting if not also (after a century of total war and the inhumanity of man to man) very deeply evocative of important human issues which rhetoric as an instrument of the moral imagination is about and tries to address for better or for worse. It is also resonant with Steve's original title, does justice to his role at the Conference as Primus Inter Pares."

What a great letter, and what powerful argument! So I wrote to James: "Many thanks for voicing an important critique and offering an interesting title. I suggest we use *Rhetoric Culture. Anthropology of the Spoken and Unspeakable* until someone offers an even better idea. But I have a question: You say that 'Anthropology of the Said and the Unsaid' repeats an earlier title and is therefore too obliging. Does this not also apply to 'The Unspeakable', which was the title for Stephen's essays published in 1987?

The problem of 'unspeakable', 'collocation' and 'concern'

I also wrote to Jean Nienkamp, telling her of Jim's objection and asking what do next. Her reply came the same day, saying: "My main hesitation about the subtitle is that it gives a lot of weight to the negative aspects of culture. "Unspeakable" has much more negative connotations than "unsaid." I know those are what James wants, but do they really reflect the constructive aspects of the theory that seem to predominate what I've read about it? –Wow – I haven't been involved in a collection project before where the title was so contested!"

I answered Jean Nienkamp immediately, this time with a long letter where I tried to sort out ideas: "I am asking your help because the question of the title is driving me crazy, and because you are, as you yourself say, a newcomer to the project and therefore may be able to assess things more clearly. And this is what you have done, you have sorted out very well what I felt but was unable to express... You are right, we want to keep and stress the positive side of our project, that is the rhetorics of imagining, wishing, longing, dreaming that achieve harmony in culture and make life worth living. At the same time we also want to stress and keep the critical side of our project which is concerned with the ordeals of culture, with the rhetorics that lead to disharmony, madness and destruction... This is the kind of contrast and polarity you are asking me to draw. But how do we express this polarity in the title? Also, is it necessary to have it in the title? I don't know and wonder what you might say. I still have the intuition that it would be good to keep the basic title "Rhetoric Culture" because this is really new and has become our trade-mark. Also, "Anthropology of..." would be very good if we could solve the riddle that has appeared since we took out 'The Said and the Unsaid'....

By the way, Stephen and I discovered the concept of 'Rhetoric Culture' in a French Café in Houston. I still see the pen in Stephen's hand as he tries to write on a napkin, gradually deleting everything that surrounded our earlier titles for a workshop we were planning for an EASA conference in Frankfurt, 1998. We deleted 'theory', 'interaction', 'relevance' etc. and 'in', 'and', 'of', 'for' and the like until we found ourselves confronting 'rhetoric culture'. Since then we have been puzzling what rhetoric culture may mean. Do you know the saying "First comes the making and then comes the matching"? Artists use it in order to express the fact that meaning is something they find out later once they have finished their work. The same applies to scholarly work like ours. So 'rhetoric culture' is first of all a collocation, and only slowly are we beginning to fathom its many implications. Therefore the most honest title would be: "Rhetoric Culture. Anthropology of a collocation", or "Anthropology of a juncture". But would people not be baffled rather than attracted by such a title?

To finish for today, I now drop the terrible 'Anthropology of....', and turn to what is entailed in your first question. You alert me to make sure that we do not raise wrong expectations, and indirectly you ask me to think of the central issues of the first volume. A short answer to this would be that part one is about 'theory' and part two is about 'concerns', whereby 'concerns' would here evoke a whole range of meanings including a view from outside (concerns of the theory, the disciplines, the scholars etc.) and from inside (concerns of people who 'grapple with the vicissitudes of life'). In other words, the title could be: *Rhetoric Culture. Theory and Concerns.* But as I look at it I find it very dry and think it does in no way cause the 'bang' with which Marion would like to open the series. One would need three terms to create at least a slight sense of excitement, like for example: *Rhetoric Culture. Theory, Concerns and Perils*. Gosh, how difficult this is! But let us not give up."

Between new and old, engagement and disengagement

Shortly after this, I sent yet another letter to Jean Nienkamp noting that now our discussion had begun to swing back and forth not only between the new and the old, but also between engagement and disengagement:

"I seem to be in a kind of 'title trance'. Day and night I keep thinking. Now you have directed me to find a title that is most true to the central concerns of all contributors. The title of the conference was *Rhetoric Culture. General Theory*. Not all the contributors addressed questions of general theory, but the questions were nevertheless in their minds. Everyone knew we were aiming at a new beginning that Stephen, Christian and I had outlined in 'What is rhetoric culture theory?' Following your advice, I began this morning with a title that would fit the aim of our first conference most closely, calling the book *Rhetoric Culture Theory. A New Beginning*. But this would not satisfy Jim Fernandez and others like Ralph Cintron, myself and probably also you, because our concern with the practical and societal relevance of rhetoric culture theory. So I added relevance to the title, calling the book *Rhetoric Culture Theory: Relevance of a new Beginning*. As soon as I had done this, I heard Ralph scream about the 'new' in the title. He recently had rightly alerted us, saying: "isn't part of the point to talk about a long history of this perspective?" I also saw Stephen laugh, for he wrote in *Meditation of Mediation*:

'Rhetoric/Culture seeks to understand this emergent return to the transcultural context of classical rhetoric. It is a kind of detour through the past in which the past is not just something to be discredited or overcome in the production of the new, but it will have been instead the means of a kind of appropriation of the future already present'.

But if it can't be a 'new beginning' what then could it be? A playful and provocative answer that would be consonant with Stephen's thoughts would be *Rhetoric Culture Theory. Relevance of an old Beginning*. Am I right that 'beginning' has connotations that also include 'doing', 'undertaking', 'project', 'plan', 'scheme', 'hope'...? My dictionary does not say so, but if these connotations were there, this would surely strengthen and give more food for thought to the title. But having reached this point, I now imagine how Marion objects: "This is not the 'bang' that I want for a title", and I imagine a telephone conversation where I offer a compromise that is not weakened by 'old beginning' and simply reads: *Rhetoric Culture. Theory and Relevance*. "

'Renaissance' interlude

A few days later I sent a further letter to Jean Nienkamp in which I said: "Thanks for bringing a whole lot of elements to the surface that have to do with our quest for finding the right title. I like that you like the paradox of 'old beginning', but I have begun to shy away from a title that may be too refined, and as I was thinking about your title thoughts this morning another idea began to buzz in my mind. Trying to answer the problems of 'old beginning', 'concern', 'relevance' and trying above all to be true to the central intention of our first rhetoric culture conference I suddenly said to myself, this is it: *Rhetoric Culture Theory. Renaissance of an Ancient Project.*

This sounds a bit more ambitious than we dared to be before the conference, but things have gone so well that we could probably use this title now for the first volume of the 'Studies in Rhetoric Culture' series, especially as we will say in the introduction and in several contributions that our conferences and our series are only part of this current 'renaissance of an ancient project'. In my imagination, 'renaissance' and 'project' overcome many of the problems with which I – and now also you – have been grappling. What do you think? Have we now arrived at a title that will last, or will there be objections? Please let me know."

Near midnight, an answer from Jean Nienkamp came which demolished 'renaissance' and showed what great fool I had been to suggest such a loaded term:

"I'm playing the devil's advocate, but of course there will be objections — (do you think you'll get this group to agree on a single title? :-) I like "Ancient Project" – but what would you think about "Re-articulating" or "Redesigning" instead of "Renaissance"? "Renaissance" seems like such a loaded term, and if you have any poststructuralists on board it will have what they might consider "unfortunate" humanistic implications. (I have never understood how the poststructuralists made "antihumanism" to be a privileged term, but I guess that's why I'm not one. I like humans, myself.)."

Also Stephen Tyler answered, and like Jean made me laugh because he didn't think much of 'renaissance': "Not too wild about renaissance, sounds like the return of the repressed, which is a nice title by itself, but too tricky."

As Jean and Stephen had relegated 'renaissance' to the trash bin I was facing a new problem: How could the sense of involvement, urgency, concern etc. be kept on which Jim had insisted. So where would we go next? I didn't know and didn't dare anymore to ask anyone. Then Jim sent me a letter that cheered me up a lot. He did not come up with yet another title, but he stressed that our long debate about the title was a good thing:

"It is a testimony to you and to the Conference that we are getting such a productive debate on the title. We are rhetoricians after all and must choose our words carefully!! Yes the "Unspeakable" carries a heavy weight and Marketers will naturally object to it. But it is not discordant with the Century of Total War and its present continuation! I like the attempt to combine old and new in the "Remembering – Renovating" suggestions. We ought to publish this enlightening struggle as an example of rhetoric culture."

Emergence and cosmos

In the letter where Stephen was "not too wild about renaissance", he also mentioned a few other possible titles in response to the question whether we should stress that the rhetoric culture project was a new or an old undertaking, he also mentioned that a key role should be allocated to the notion of emergence. He wrote: "I don't much like theory titles or 'new', though somehow it would be nice to incorporate those ideas without saying so directly. How about 'emergence and convergence in the discourse of... (in social science????...) discourse'. An 'emerging synthesis' ??? Emerging syntheses of discourse (in, for)?... Something awful: 'the analysis and interpretation of an emerging order of discourse (emergent orders of discourse).' Don't know if any of this is helpful or merely more confusion. Let me know."

Next day I pondered Stephen's interest in emergence and co-emergence. Earlier on, when Christian and I were in a very enthusiastic and reckless mood, I had suggested that the rhetoric culture project was ultimately concerned with exploring the 'galaxies of discourse'. This concept would go well with the ICP model projected on a galaxy that we had used as the main poster for our first two rhetoric culture projects and that I imagined for the cover of the first book in the Berghahn series 'Studies in Rhetoric Culture'. Stephen liked this hyperbolic title: "I have taken a liking to the idea of galaxies and think "Galaxies of Discourse" as a subtitle is very nice, indeed. I like "exploring," too. Maybe "navigating galaxies" ??? maybe "intergalactic," or "emerging (emergent) galaxies " or "the role (study of, function, structure) of emergent," galaxies of discourse." Or if new is important, "emergence of new galaxies of discourse in..."

I had forwarded Stephen's ideas to Jean Nienkamp and she replied by sending the following title thoughts: "Too tired to contribute tonight (Creating Cosmos Through Discourse. Rearticulating The Discursive Cosmos. Cosmography of an Ancient Project. I told you.) – but will try later."

I liked the first title and wrote back: "*Rhetoric Culture. Creating Cosmos Through Discourse.* What a wonderful title! It is not convoluted and idiosyncratic like some of the earlier, present and future titles. No! It is sound, round, open, inviting, harmonic and would go well with the galaxy and the ICP model that I would like on the cover."

Jean Nienkamp answered by drawing attention to the question whether we want the subtitle to the original rhetoric-culture process or our analysis of it: "Well, I'm glad you like it – Actually, I came up with that one the night we talked on the phone, not last night when I was tired. My concern is that the subtitle refers to the original rhetoric-culture process, not necessarily to our analysis of it, but maybe that's what we want. I hope your colleagues like it.

Next day I had a long telephone conversation with Marion Berghahn. She found that 'cosmos' and 'creating' were too biblical, too harmonic and did not reflect the hard, antagonistic and devilish side of rhetoric and culture. Also Christian and others found the cosmic track too wayward and objected. Jean Nienkamp and I agreed as soon as we heard of these objections. 'Cosmos' and 'galaxies' were too far out there, images that led away from our most immediate concerns, that is people talking to one another, – willfully, intentionally.

Before, behind and beyond words

Shortly after Jean Nienkamp had sent me her most recent title thoughts, Jean Lydall sent me a letter from Australia, and at the end she added some title ideas for our collection. Most of them were just teasing and meant for fun, some were odd, but one I liked especially well. This was "Rhetoric Culture: Before, behind and beyond words".

I thought that Jean Lydall's title suited the topic of the 'said and the unsaid' especially well, and 'danced around' *Rhetoric Culture* in threefold steps that mirrored Stephen's ICP model. Also, the title marvelously escaped the danger of complacency, for *Before, Behind and Beyond Words* applies to the thinking and writing that go into the book as well as to the 'cultural realities' that are the subject of the book. There is no division here into a world of scholars who know and a world of every-day life that does not understand. The title would be great because it allowed one to read some further wisdom in it, i.e. a kind of irony that says that both cultural theory and practice have no secure semantic grounding. Also, 'before, behind and beyond' may lead one to evocations of time and space, maybe even of a field of fluctuating forces, a field of rhetorical energy and so on. In other words, part of the strength of Jean Lydall's title would be that although it may sidestep some central issues, it was open to a whole lot of productive evocations. Furthermore, as Christian pointed out, the title signaled openness and went well with the fact that the first volume in the 'Studies in Rhetoric and Culture' marks a beginning. Finally, it accorded very well with Stephen's favourite metaphor for our project as a kind of 'journey'.

But even though I liked the 'words' title, I kept wondering whether it was really what we wanted. In the end I decided to once again send a circular letter to all contributors. It read as follows:
"Dear Contributors to Rhetoric Culture Volume I,

We still have to agree on the subtitle for the first volume in our "Studies in Rhetoric and Culture" series that will be published by Berghahn Books. There has always been consensus that the first volume should have *Rhetoric Culture* in its title, for the first volume has the function of opening up our central theme, of beginning something that only later will find its full completion. Our use of *Rhetoric Culture* is here analogous to Jean Nienkamp's use of *Internal Rhetorics*. Jean says in the opening sentences of her fascinating book: "The biggest leap I take in this book is the title, *Internal Rhetorics*, by which I initiate a study of the persuasive techniques we use on ourselves. The term is intended to be obvious and paradoxical - and, by the end of the book, complex." *Rhetoric Culture* similarly involves a thematic 'leap', and while RC may not be as paradoxical as IR it is nevertheless puzzling and makes us look forward to the many complexities it will have in store for us. I think we all agree that we need a subtitle that goes well with the mental and emotional currents and movements inherent in *Rhetoric Culture,* and this is why we are having such a prolonged search. You find a chronicle of this search as attachment, but before you read it, think and decide about the most recent title proposal that comes from Jean Lydall: *Rhetoric Culture. Before, Behind, and Beyond Words.* Please let me know whether you like it or want to propose another one."

I sent out the circular in the morning, and on the same day first answers came in:
Jean Nienkamp, "I'm absolutely not going to have any suggestions or complaints about "Before, Behind, and Beyond Words" or you'll never get past the title discussion!"

Ralph Cintron, "I too think the search for an appropriate title has been a most interesting process. I've read through the entire exchange & I like the spirit of it. Before, Behind, & Beyond Words is a good subtitle. One of the things that has interested me a great deal, of course, is to think of rhetoric "beyond" language, so for me built environments are rhetorical, animals communicate rhetorically, plants may also, & things that happen in the cosmos become "signs" that speak to humans. For instance, how many people read the Challenger crash as a portent of what would happen to the US if it entered militarily into the Mideast? Parts of the Challenger fell in an area called Palestine, Texas & all this is not too far away from Bush's ranch in Texas. So getting the notion of "beyond" in there is important";
Brigitte Nerlich, "What about Rhetoric Culture Vulture?! Joke. But what about: How to create worlds with words: Investigations into rhetoric culture??? Final offer!!!"

From what Jean, Ralph and Brigitte said, I gathered that they were not really enthusiastic about *Before, Behind and Beyond Words*. Jean Nienkamp agreed only because she had given up the hope that we will ever find a title to which everyone agreed. Ralph was not happy to loose the notion of cosmos that Jean Nienkamp had proposed, and Brigitte, like Jean Nienkamp, wanted 'creation' in. I thought a bit about this and then called Brigitte proposing the following collocation: *Rhetoric Culture. The Creation of Worlds with Words*. She was delighted and answered: "Yes, this is IT!"

I thanked Jean, Ralph and Brigitte immediately and sent them the most recent part of this chronicle. But even though I liked the 'wor(l)ds' part, I found that now the sense of duress, trouble, agony, war etc. was going amiss again. So I wondered what kinds of comments would come from the other contributors.

Jean Nienkamp answered: "I didn't know that my refusal to comment sounded so negative! I was so tempted to tweak and tweak, and then I thought 'no, at that rate we'll never settle on a title.' I do like your new title proposal, genuinely, no reservations. It resonates with Paulo Friere talking about teaching the word and the world."

Some comments, were also positive while at the same time proposing certain changes like Suzanne Kemmer's: "I have just read your piece on the title of Volume 1 of the *Rhetoric Culture* series. Mind-boggling or mind-dazzling, I'm not sure which. But the final paragraph made a pinpoint of light emerge in my own mind, that began to grow. It is just a slight variant on one of the Jeans' suggestions. What about simply, *Beyond Words*? This evokes both the positive and the appalling senses of Unspeakable. It is simple, and multiply ambiguous and metaphorical. Adding more prepositions, I think, doesn't add to this, it just overplays it."
And Ellen Basso wrote: " I got about half way through your narrative of the title discussion, and then it came to me (from your own letter, below): "Rhetoric Culture: Anticipating a Complex Debate". Well, it's not really very good but it might push us in new directions, away from the cosmic, in any case."

But others rejected the title completely. Francoise Douay wrote, "I hate to confess that "Before, Behind and Beyond Words" sounds laughable to me, as if we were interested in "anything but words", I mean "everything around words except words themselves": do we really wish to advertise that?" and Michael Carrithers laconically let me know, "I don't like that title much, since 'words' doesn't seem to capture it for me."

Ubiquity

Michael had written to me earlier, saying, "I've been thinking up all sorts of subtitles, and I don't like any of them, but will say this: something with words like image, imagination, figure, figurative, resonance, weaving, singing, moving, persuading, convincing, picture, picturing, action have all recommended themselves to me." And I had answered that I had gone on a bicycle ride and had kept coming up with other subtitles according to the various contents of Volume I that I wanted to stress.

Michael had replied: "Well so far the one I like, actually a good deal more than 'the said and the unsaid', is *The Ubiquity of Persuasion in Social Life.* I admit now that I approved of the earlier idea largely because I rather liked that book too. But on mature reflection, I think it is too little explanatory, and that the bare title 'Rhetoric Culture Theory' would be better than what would probably be a rather obfuscating subtitle. *The Ubiquity of Persuasion in Social Life* is at least a pretty faithful rendition of the project. I had something in the middle of the night (probably a lousy idea for that very reason), and will try to capture it as I go about my day."

Willful culture

After this, I returned to my desk – almost with despair. I urgently needed a title, for otherwise I could not write a proper book outline for the publisher. Marion Berghahn would want punch lines that told of the thrust and the main direction of the book. So, what was its central theme, or at least the theme that we should emphasize most? There were so many directions, and we had tried so many of them. Jean Nienkamp, our expert on 'internal rhetorics', had already warned me that if I went on like this I would get stuck and never get passed the title discussion. So, in a renewed effort to find a solution I recalled the main objections that had been made so far against various titles:

1. Felix Girke was against usurping 'theory' for the first volume.
2. Jon Abbink found 'exploring' much to weak.
3. Ralph Cintron managed to debunk 'paradigm'.
4. James Fernandez saw problems in repeating 'the said and the unsaid'.
5. Jean Nienkamp warned against ' the unspeakable'.
6. Jean Nienkamp demolished 'renaissance'.
7. Ellen Basso urged us to get away from the cosmic.
8. Francoise Douay argued against a focus on 'words'.

Our point of departure had been the hope to contribute to a new direction in anthropology. This direction was expressed in the tantalizing collocation of 'Rhetoric Culture', without hyphen, slash or any connectives like 'and' or 'in'. In many ways, 'Rhetoric Culture' had already become the trademark of our project and figured strongly in our previous conferences. Maybe the title search had shown that there were still too many different themes and concerns in the first volume. Would it perhaps be better to split the original Volume I into three, one focusing on general theory, one dealing with ordeals and vicissitudes and one on resonance and ethnography? How would such a new composition look like? I tried this out and failed completely. No, this could not be the answer.

I was still left with a number of options that I, or rather we, had not yet pursued fully. One of them was the notion of 'will' that is so central to Stephen's work and, of course, to rhetoric in general. I had already thought of titles that had 'willfulness' in them and after trying out a number of possibilities that I won't repeat here, I came up with the following: *Rhetoric Culture: The Willful Forms of Human Life*. But had 'willful' - like 'unspeakable' - not too many negative connotations? No, our contributors would never accept this title.

Theory, concerns and implications

Pierre Maranda had once written: "Yet, I so much like Rhetoric Culture Theory". So I asked myself how we could smuggle in 'theory' without Stephen minding it too much, and I wrote yet another circular letter saying: "The last circular letter led to a whole lot of exchanges and the many title thoughts that you find in the attachment. Don't bother to read it if you are fed up with the seemingly endless story of our title search, but please let me know whether you would find *Rhetoric Culture. Theory, Concerns and Implications* acceptable. I urgently need a title, for otherwise I can't write a proper book outline for the publisher. Marion Berghahn wants good punch lines, but as long as the title of the book is still in doubt I find it impossible to summarize the main thrust and direction of the book."

Two answers came in immediately. Jean Nienkamp asked, "Are you getting fed up with the title search? The one you've suggested here hints of exhaustion. Would it be mean of me to offer a final tweak? What I did like was the inadvertent juxtaposition of "Anticipations" and "Implications" in your message. I wonder if Berghahn could do something typographically cool to make those two words symbiotic on the cover? The inside subtitle could then be something like *Anticipations,* *Implications*. Less explicit than what you've suggested, but I think that *Rhetoric Culture* itself is quite explicit. Anyhow, no need to pass this on if you just want to have done with it and move on. You can't spend your whole sabbatical on a subtitle!"

Michael Carrithers wrote: "Well, I did prefer the subtitle *The Ubiquity of Persuasion in Social Life*, which seemed to me to get to the point of culture-as-rhetoric, and for me the point of the whole wonderful enterprise. But in this suggestion, which seems to change the title to *Rhetoric Culture: Theory, Concerns, and Implications*, that specificity has been lost. BUT I would not want you to wander in the woods much longer. Organising academics is like trying to herd cats. This present proposal has at least the advantage of seeming ambitious, and seeming even more general, if a good deal vaguer. So, though I preferred the other suggestion more, I'll heave a sigh and go with this if necessary. Yet, alas, I cannot forbear to note that you could lose the word 'concerns' and it would at least be a more effectively ambitious title: *Rhetoric Culture: Theory and Implications*.

Coming full circle

Michael added a further – decisive – comment, saying, "I see, yes, that persuasion alone may be too narrow a view on rhetoric, and I have much liked the ideas of resonance etc., and indeed think of exploring persuasion-as-resonance, the sort of thing you see in evangelical Christian meetings. But I also thought that the phrase 'rhetoric culture theory' had something appropriately grandiose, ungainly and rhetorically pretentious about it, a claim, an ambition, and a probably appropriate awkwardness that at least the insider could regard with fond irony. I do enjoy your account of the search for a title. Very illuminating. I have somewhere the story of the blind men and the elephant...do you know it? Says one blind man: It resembles a tree trunk! Says another: a broom! A third: a snake! And so forth."

What a wonderful thing of Michael to say! With this it seemed that we had come round circle and were back to where we had started. My old favorite title *Rhetoric Culture Theory* to which Felix had objected was perhaps the best after all. Hadn't we used it already successfully to apply for funds from the Volkswagen Foundation and for inviting scholars from all over the world to our conference in Mainz, February 2002? When I told Jim Fernandez that in my view we had come round circle, he answered: "Reculer pour mieux sauter."

*** *finis* ***

MOTHER TONGUE
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Autobiography of a Lucky Man

Now that I near eighty years of age I must comment in the most general terms on my life because at this age one never knows how much time there remains to think about the most salient characteristics of one's life. How did I manage to live so much longer than my mother, my brother, and my two sisters? Even my father only made it to 78 and he was sick for quite a while before he died. Since I am fairly healthy, we can expect that I will eventually exceed his life span by quite a bit. So what is the answer?

Two answers spring to mind. First, I am a beneficiary of modern medical science. At crucial points the doctors kept me alive where they failed the rest of my family. Second, it is a matter of my having the good genes that the others missed. After all most of my maternal aunts and uncles lived deep into their 80s and my Aunt Ada lived to be 95. And my Scottish great-grandmother lived to the same age and even kept her teeth. All this before medical science got so good. But since a friendly endocrinologist in Pittsburgh got interested in this problem, he had me tested for a known gene which kills with heart disease, high blood pressure, and stroke**; I clearly had that gene**, the same one that definitely killed at last three of my family. So the genes don't deserve the credit. Furthermore, how did I ever live long enough to be alive when medicine got good?

Let us call it luck, good fortune. At crucial times when death, or at least non-existence, loomed, I got lucky. I was not supposed to be born because my mother could not stand her abusive husband and planned to move to California without him. She made the wrong decision and stayed. She loved him! Her reward was a divorce a few years later and a sad unhappy life from then on. But she bore the wretch two more children, one of whom was lucky me. But fate also included a very loving mother to grow up with. The third bit of luck was the fact that I was born into a rich man's household – good care, good food, shelter, the whole bit that comes with wealth. Lots of people did not have those things in their childhoods. As friends from other ethnic groups told me later it was also luck to be born into the dominant ethnic group, New England Yankee. Although father was a Scot, that did not hurt either. Luck also included being the baby of the family. The daddy abused my brother, the eldest of the children, and so terrified or dominated the older sister that she never recovered an ability to love and never lost her anxiety. Yet the mother who could not protect all her children managed to protect the youngest, the two babies she got from her unfortunate decision and especially the baby boy. To their dying days the elder two never let me forget how lucky and spoiled I was. Of course they were right!

From 1929 to 1932 the family fell apart the same way the American economy did. Rich father lost his shirt and descended from the upper class to the lower. Mother got a good settlement from him before he lost it all but she invested it foolishly and so lost it too. She worked for the next 15 years as a sales girl, supporting her two youngest by herself – the single mother syndrome. Ah, but there was luck in our lives. Mother moved to her home town with many good kind kinfolk all around. Their love and moral support made a big difference to all of us and it was a very practical thing to have them around during the depths of the Great Depression.

During the divorce episode I flunked out of kindergarten because I hated to read, so I started school anew in Winsted (mother's home town), albeit a year older than other kids. We lived in a friendly Lebanese's apartment building and there the next great piece of luck occurred. Across the street from the apartment was a good public library with circa 20,000 volumes. Where could a lonely little boy with no money and no friends hang out after school? That's right, the library. It took very little time to get over the hatred for reading because some kind of paradise had just opened up. The library was rich in history books, geography books, historical novels and stories about whatever. Thus was born the makings of an anthropologist with an interest in historical approaches. Winsted with its many ethnic groups living harmoniously together provided the other element, an interesting society or the last element needed to make an anthropologist. From the Lebanese (called Syrians locally) and predominantly Sicilian Italians the concepts of culture and language took root because the local differences were palpable. And finally the urge to make sense of all these things, and the town itself, in later years surely pointed to the social sciences. What could be more fun to study?

As a growing boy, I had the good fortune to live in a peaceful town; no gang fighting, no violence, no drugs, no prickly ethnic animosities, no 'out of bounds' areas, no glaring social class differences. Barely any African-Americans and very few Jews were present to illustrate caste differences. In college I learned that Jews were virtually a caste group. That is no longer true and it never was in Winsted due to scarcity no doubt.

Once while learning to swim at the local lake I nearly drowned but some kind person rescued me. Another time my mother got me to look inside a smelly oven (natural gas); no light but a wooden match. The explosion blew me clear across the room but no injury. It wasn't until I was 17 and working in a local factory that luck took a serious turn for me. Operating an open elevator which carried freight from floor to floor, I got my head caught in the elevator which proceeded to behead me or try to. At the last moment I realized that I was pulling the up/down cable which caused the elevator to move. With great relief I stopped pulling the cable down and so retrieved my head. Phew! But the lucky part came a second later when, to my horror, another worker on another floor pulled the cable and the elevator went back down but without my head!

At age 18 extraordinary luck arrived. Drafted into the Navy in early 1945, I was trained in the amphibious forces which were meant to invade Japan later on that year. We were told that we radio operators were likely to be killed during landing operations – we were conspicuous and important to knock out. Thus we were jubilant when the USA dropped the atom bomb on Japan and ended the war. For the world it was probably a great tragedy in the long run but for me it was a piece of luck. This was not a great day for mankind; we all knew that and we felt more than a little guilty but personal joy at survival could not be stifled.

Two years later, due in part to that last piece of luck, I entered college on the GI Bill of Rights. Four years of college FREE or, more precisely, paid for by the American people. Wow! I probably would not have been able to go to college without that Bill. Additional luck came when, because I was a veteran, the college did not take my high school grades into consideration. I got into Yale on the strength of a special aptitude test for veterans. Just to make this point – I tried Harvard which had no such special test and they rejected my application scornfully. "Why don't you go to Ohio State?" they said. (Harvard could be nasty in those days.) Since no other generation has been treated so well by a grateful country, I count this as a big chunk of luck.

Hard work can account for most of the career as an anthropologist, although luck figured in my admission to graduate school at Yale. My senior advisor saw me as replacing his son who rejected anthropology to become an insurance agent. So, despite my numerous faults and flaws, he got me accepted **on probation**. Hence he got me in the door to a scientific career. The rest was just hard work. With a wife, a child, and a skimpy scholarship I worked 20 hours a week for a land surveyor, also attending classes, writing papers, etc. Yep, hard work!

Still fate wanted to terminate me in my late sixties. Right, that bad gene! Thereupon, when I came home from field work in Ethiopia (at age 64), the medics found a heart murmur which I had known since age 14 but 'lost' in between. "You need surgery for that leaky valve! Do not wait long to have it done!" said the doctors. "Remember that 97% of patients survive this operation; it's almost a cinch".[1] So two years later (1992) we had the surgery and it came within a snitch, a gnat's eyelash, of killing me. Before my wife chased him away a Catholic priest had started the "last rites" of the soon to die. My daughters started phoning relatives and friends with the news of my demise. At the last second (almost) a new medicine worked its wonder and they dragged me back from the abyss. Afterwards the doctors told me it was a **miracle!** When I thanked them, they bowed slightly but retorted that the miracle had been possible only because of my wife's intervention in their treatment of the case. How so? Because it was a grand holiday (Thanksgiving) the doctors did not wish to work that day. My Nancy descended on them like Queen Boudicca on the Romans! "If this man dies, you better get ready for a massive malpractice suit! It is clear that you screwed up!" Thus motivated, the doctors worked feverishly! So we all won –just barely. The miracle was indeed fortunate but really the biggest luck was in having such a stalwart wife who overlooked my flaws and faults to give me one more chance and who worked skillfully to get results.

As it turned out the bad gene was not responsible for that incident. The leaky valve was a birth defect. The gene would have killed me anyway six or seven years later but by then medical science could curb the gene's appetite. I guess another piece of luck.

Buona fortuna! La fortuna è buonissima!

Ma ... La vita è straordinariamente strano !

E un poco ridicolo, no?

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[1] 'Cinch' here means something very easy, a sure thing.

MOTHER TONGUE
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The Glottalic Theory of Proto-Indo-European Consonantism and Its Implications for Nostratic Sound Correspondences

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1. Historical Background

In 1903, the Danish linguist Holger Pedersen was the first to observe that certain languages/language families of Europe, Northern and Eastern Africa, the ancient Near East (including the Caucasus Mountains), Northern Eurasia, and India might be genetically related. Though he never published a systematic account of his views, he did make the following remarks (1931:335—338):

> The question of the relationship among the Indo-European and foreign families of languages came up in the first period of comparative linguistics. Relationship between Semitic and Indo-European was asserted by Rudolf von Raumer, beginning in 1863, and by Ascoli from 1864 on. But convincing proof could not be expected at that time. Resemblances in the morphology of the two families are extremely few, and proof by means of vocabulary and the laws of sounds was not then understood. Schleicher denied most positively any relationship between the two, pointing to the great dissimilarity in the forms of the roots: in Semitic the roots consist of three syllables of very simple and uniform structure, as in Arabic *ḳatala* (root form and preterite of the verb 'to kill'), while in Indo-European the roots are monosyllabic and of widely varying — partly heavily compounded — form, as in Latin *ī-re* 'to go,' *stā-re* 'to stand,' *lub-et* 'it pleases,' *vert-ō* 'I turn,' *ed-ō* 'I eat,' and so on. At that time nobody could weaken this argument. And it might have been added, although Schleicher did not do so, that the phonetic systems of the two language families are extremely different, as may be seen from a single example: in Semitic there is an abundance of gutturals, whereas in Indo-European there is not one, not even the (to us) ordinary *h*. With this in view, one might feel tempted to assent to Schleicher's exclamation: "What weight have the few similarities in roots in the two language families against these sharp contrasts?" And one might well be disposed to neglect "the few similarities" which one could not help observing.
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> Nothing was changed in the problem by the first step in a systematic examination of the vocabulary which Friedrich Delitzsch took in his *Studien über indogermanisch-semitische Wurzelverwandtschaft* (1873) But the development of Indo-European linguistics changed the problem greatly. The monosyllabic form of Indo-European roots turned out to be an entirely secondary phenomenon: in historical times the roots of the words for *heaven*, *god*, or *heart* may appear to be **diw*- or **ḱerd*-, but we have good reason to believe that in the period older than that of the Indo-European parent language these roots had forms like **däyäwä*-, or **ḱärädä*- …, and that the phonological system in this older period had quite a different appearance from that which we attribute to the Indo-European language.
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> With this background, there appeared in 1906 an extraordinarily important work by the Danish scholar Hermann Möller, *Semitisch und Indogermanisch*. This is a splendid attempt to discover the laws controlling the relationship between Indo-European and Semitic consonants — a successful attempt, although only the main lines of

development are traced. Time alone will show how far we can advance by Möller's method. Certain it is, however, that the comparison of the two families can never be carried out so completely and in such detail as the comparison within the fields of the individual languages of one family.

But Indo-European has been brought into connection with other families besides Semitic. Vilhelm Thomsen, as early as 1869, indicated the possibility of a relationship with Finno-Ugrian, but he did not pursue the subject very far. In 1879, the Estonian Nicolai Anderson published an extensive work on the subject, the value of which is considerably impaired by its many errors. Great interest was awakened when the English scholar Henry Sweet advocated the relationship somewhat passionately in a little popular book, *The History of Language* (1900). However, among the individual similarities which Sweet mentions, some are incorrect, and his space was too limited to permit of actual proof. Trustworthy studies of some length by K. B. Wiklund and H. Paasonen appeared in 1906 and 1908. After these works it seemed unnecessary to doubt the relationship further.

Moreover, the inflectional systems show much greater relationships than in the case of Semitic. The original ending of the accusative case in Finno-Ugrian was *-m*, which in Finnish has changed to *-n*. The same ending is Indo-European:

Finnish	*Cheremissian*	*Latin*	*Greek*
Nominative *käsi* hand	*kit*	*vespera* evening	*hespérā*
Accusative *käde-n*	*kiđ-əm*	*vespera-m*	*hespérā-n*

The similarities in the personal endings of verbs are especially striking:

Finnish	*Cheremissian*	*Greek*		*Sanskrit*
1st person sg. *kuolen*	I die *kole-m*	*é-phero-n*	I carried	*a-bhara-m*
1st person pl. *kuole-mme*	we die	*e-phéromen*	we carried	
2nd person pl. *kuole-tte*	you die	*e-phére-te*	you carried	

Furthermore, there is an unmistakable similarity between the two families in a series of pronouns and in the negation 'not':

Finnish	*Latin*
minä I (Lappish *mon*)	*mē* me
sinä thou (*s* from *t*; Lapp. *don*)	*tē* thee

	Sanskrit
tä-mä this	*ta-*
jo-ka who, which (relative)	*ya-*
ku-ka who? (interrogative)	*ka-*

Hungarian	*Old Norse*
ne not	*ne* not

It is impossible to regard all this as the result of accident. It is noteworthy, however, that the similarities hitherto pointed out in the more concrete part of the vocabulary are very few, although some of them are as striking as Finish *nimi* 'name,' and Latin *nōmen*. Consideration of the problem whether sound-laws still unknown to us, or morphological developments not yet understood, have obliterated the originally more numerous points of similarity, or whether the vocabulary in one of the families was largely renewed after the period in common, we must postpone until later. But to deny relationship between the families would be overbold.

> If we accept relationship, we are led yet further afield, not only to Samoyed, which cannot be separated from Finno-Ugrian, but throughout all of Northern Asia and across the Bering Strait, because similar, though fainter, resemblances like those here cited are found also in Turkish, Mongolian and Manchu, in Yukaghir, and even in Eskimo. If, on the other hand, we agree in the matter of relationship with Semitic, then we must also accept relationship with the far-flung Hamitic family, and perhaps with Basque. And squarely in the midst between our supposed Northern and Southern relatives stand the Caucasian languages, which we cannot ignore, and various extinct languages in Asia Minor and thereabout. It is not impossible that some of the non-Indo-European languages of antiquity in Asia Minor were once most closely related of all to the Indo-European family.
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> As a comprehensive designation for the families of languages which are related to Indo-European, we may employ the expression *Nostratian Languages* (from Latin *nostrās* 'our countryman'). The boundaries for the Nostratian world of languages cannot yet be determined, but the area is enormous, and includes such widely divergent races that one becomes almost dizzy at the thought.

As can be seen from the above remarks, Pedersen had a good sense of which languages/language families might be related, though Basque should not be included among these.

Pedersen's insightful remarks notwithstanding, relatively little work was done during the first half of the twentieth century on distant linguistic relationship, and the little work that was done was not of high quality and did more to discredit the endeavor than to help. Gradually, the intellectual climate, especially in the United States, became hostile to long-range comparison.

Beginning in the mid-1960's, the intellectual climate slowly began to turn around, and a growing number of linguists, especially in the former Soviet Union, began to turn attention toward investigating distant linguistic relationship. The revived interest was sparked by the work of Vladislav M. Illič-Svityč [Иллич-Свитыч] and Aaron B. Dolgopolsky [Долгопольский], who first started working independently and, at a later date, through the efforts of their mutual friend Vladimir Dybo [Дыбо], cooperatively. Their work, though not without its own shortcomings, was the first successful demonstration that certain language phyla of northern and central Eurasia, the Indian subcontinent, and the ancient Near East might be genetically related. Following the proposal made in 1903 by Holger Pedersen, they employed the name "Nostratic" to designate this grouping of languages. In particular, Illič-Svityč, in the course of several publications, culminating in his posthumous comparative Nostratic dictionary included Indo-European, Kartvelian, Afrasian (also called Afroasiatic [Afro-Asiatic], Hamito-Semitic, or Semito-Hamitic), Uralic, Dravidian, and Altaic in his version of the Nostratic macrofamily. From his very earliest writings, Dolgopolsky also included Chukchi-Kamchatkan and Eskimo-Aleut.

Before his tragic death in an automobile accident on 21 August 1966, Illič-Svityč had planned to prepare a comparative Nostratic dictionary listing over 600 Nostratic roots and tracing their development in detail in each of the daughter languages in which they were attested. He had published a preliminary report on his work in 1965 entitled "Материалы к сравнительному словарю ностратических языков (индоевропейский, алтайский, уральский, дравидский, картвельский, семитохамитский)" [Materials for a Comparative Dictionary of the Nostratic Languages (Indo-European, Altaic, Uralic, Dravidian, Kartvelian, Hamito-Semitic)]. Working diligently, literally devoting all of his energy to the project, he had managed to prepare the entries for approximately 350 roots. After his death, Illič-Svityč's work was prepared for publication by the dedicated efforts of Rimma Bulatova, Vladimir Dybo, and Aaron Dolgopolsky, with the result that the first volume of the dictionary appeared in 1971, containing 245 entries. A second, smaller volume appeared in 1976, listing entries 246 through 353 and ending with an index — this completed all of the material prepared by Illič-Svityč himself (by the time this volume appeared, Dolgopolsky was in the process of emigrating to Israel). Finally, the first fascicle of volume three appeared in 1984, containing entries 354 through 378, none of which was prepared by Illič-Svityč — it represents the collective efforts of a team of scholars.

In the meantime, Dolgopolsky has continued to make important contributions to Nostratic studies, especially a 1984 paper on Nostratic pronouns and a 1998 book entitled *The Nostratic Macrofamily and Linguistic Palaeontology*, and currently has material to support the reconstruction of approximately 3,000 Nostratic roots. Unfortunately, only a small amount of this material has been published to date, though it is hoped that his *Nostratic Dictionary* will soon appear in print. The manuscript is finished and is in the hands of the McDonald Institute for Archaeological Research at Cambridge University.

Beginning with an article that appeared in *Orbis* in 1975, I published several studies, culminating in a 1984 book entitled *Toward Proto-Nostratic: A New Approach to the Comparison of Proto-Indo-European and Proto-Afrasian*, in which I tried to show that Indo-European and Semitic (later expanded to include all of Afrasian) might be distantly related. Reviews of this book as well as discussions with colleagues prompted me to expand the scope of my research to include other language families. This resulted in the publication in April 1994 of a joint monograph by myself and John C. Kerns entitled *The Nostratic Macrofamily: A Study in Distant Linguistic Relationship*. It was Kerns who prepared the chapter dealing with Nostratic morphology. This book supplies a great deal of lexical evidence from the Nostratic daughter languages to support the reconstruction of 601 Proto-Nostratic roots. In an article published in *Orbis* in 1995, I supplied material to support an additional 29 Proto-Nostratic roots, and another 21 etymologies were proposed in my 1996 book entitled *Indo-European and the Nostratic Hypothesis*. I have continued to work on these issues and have just completed the manuscript for a two volume, 1600-page work entitled *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology, and Vocabulary*.

The late Joseph Greenberg has prepared a two-volume work entitled *Indo-European and its Closest Relatives: The Eurasiatic Language Family*. The first volume, which was published at the beginning of 2000, deals with grammar, and the second, which was published at the beginning of 2002, deals with lexicon. Greenberg includes Indo-European, Uralic-Yukaghir, Altaic (Mongolian, Chuvash-Turkic, and Manchu-Tungus), Japanese-Korean (Korean, Ainu, and Japanese-Ryukyuan), Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut in his Eurasiatic language family. Unlike Illič-Svityč, Dolgopolsky, and myself, he does not include Kartvelian, Afrasian, nor Elamo-Dravidian — not because he believes that they are unrelated, but because he believes that these three language phyla are more distantly related to Indo-European than are the others, which, along with Indo-European, form a natural taxonomic subgrouping. My own opinion is close to that of Greenberg. As I see the situation, Nostratic includes Afrasian, Kartvelian, and Elamo-Dravidian as well as Eurasiatic; in other words, I view Nostratic as a higher-level taxonomic entity. Afrasian stands apart as an extremely ancient, independent branch — it was the first branch of Nostratic to separate from the rest of the Nostratic speech community. Younger are Kartvelian and Elamo-Dravidian. It is clear from an analysis of their vocabulary, pronominal stems, and morphological systems that Indo-European, Uralic-Yukaghir, Altaic, Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut are more closely related as a group than any one of them is to Afrasian, Kartvelian, and Elamo-Dravidian, and this is the reason that I follow Greenberg in setting up a distinct Eurasiatic subgroup within Nostratic.

2. The Nostratic Sound Correspondences of Illič-Svityč and Dolgopolsky

Illič-Svityč did not prepare a table of Nostratic sound correspondences himself, but the work was done for him by his friend Vladimir Dybo and included at the beginning of volume 1 (pp. 147—171) of Illič-Svityč's posthumous Nostratic Dictionary, *Опыт сравнения ностратических языков (семитохамитский, картвельский, индоевропейский, уральский, дравидийский, алтайский) [An Attempt at a Comparison of the Nostratic Languages (Hamito-Semitic, Kartvelian, Indo-European, Uralic, Dravidian, Altaic)]* (3 volumes, Moscow: Nauka [1971—]). The following table is taken from p. 147 of this dictionary and includes only the stops:

Nostratic		Afrasian (Afrasian)	Kartvelian	Indo-European	Uralic	Dravidian	Altaic
Initial	Medial						
p‘-		p	p, ṗ	p	p-	p-	p‘-
	-p‘-	p	p	p	-pp- ~ -p-	-pp- ~ -p-	-p- ~ -b-
p-		p_1	p_1 (p ~ b)	p ~ b	p-	p_1- (p- ~ v-)	p-
	-p-	p_1	p_1 (p ~ b)	p ~ b	-p-	-pp- ~ -v-	-b-
b-		b	b	bh	p-	p-	b
	-b-	b	b	bh	w-	-?- ~ -v-	-b-
ṭ-		ṭ (t)	ṭ	t	t-	t-	t‘-
	-ṭ-	ṭ (t)	ṭ	t	-tt- ~ -t-	-t(t)-	-t-
t-		t	t	d	t-	t-	t-
	-t-	t	t	d	-t-	-t(t)-	-d-
d-		d	d	dh	t-	t-	d-
	-d-	d	d	dh	-δ-	-ṭ(ṭ)-	-d-
ḳ-		q (k)	ḳ	k̂, k, k	k-	k-	k‘-
	-ḳ-	q	ḳ	k̂, k, k	-kk- ~ -k-	-k(k)-	-k- ~ -g-
k-		k	k	ĝ, g, g	k-	k-	k-
	-k-	k	k	ĝ, g, g	-k-	-k(k)-	-g-
g-		g	g	ĝh, gh, gh	k	k-	g-
	-g-	g	g	ĝh, gh, gh	-γ-	-:Ø-	-g-

In his forthcoming *Nostratic Dictionary*, Dolgopolsky proposes the following Nostratic sound correspondences for the stops (§2.1 — the pages are not numbered):

Nost.	Sem.	Eg.	Berber	Kart.	IE	Uralic	Turk.	Mong.	Tung.	Drav.
*b-	*b	b	*b	*b	*b^{h}	*p	*b	*b	*b	*p
*-b-	*b	b	*b, *β	*b	*b^{h}	*w, β_/*p	*b	*b	*b	*v
*p-	*p	f	*f	*p	*p, *b	*p	*b, *p˺	*φ, ?*b	*p	*p
*-p-	*p	f	*f	*p, ?*b	*p, *b	*p, ?*w	*Ø	*φ > *ɣ	*b	
*ṗ-	*p	p	*f	*p, *ṗ	*p	*p	*h > *Ø	*φ	*p	*p
*-ṗ-	*p	p	*f	*p, *ṗ	*p	*p	*pp	*p, *b	*b	*pp
*d-	*d	d	*d	*d	*d^{h}	*t	*ȷ	*d, _i/*ʒ́	*d	*t
*-d-	*d	d	*d	*d	*d^{h}	*δ	*δ	*d	*d	ṭ/ṭṭ
*t-	*t	t	*t	*t	*d	*t	*t˺	*d, _i/*ʒ́	*d	*t
*-t-	*t	t	*t	*t	*d	*t	*t	*d	*d	*ṭ
*ṭ-	*ṭ, *t	d	*d̮	*ṭ	*t	*t	*t‘	*t, _i/*ć	*t	*t
*-ṭ-	*ṭ, *t	d, t	*d̮, *t	*ṭ	*t	*tt	*t‘	*t	*t	*tt/t
*g-	*g	g, ʒ	*g	*g	*g^{h}, *ĝ^{h}, *g^{wh}	*k	*k˺	*g, *ɡ	*g	*k
*-g-	*g	g, ʒ	*g	*g	*g^{h}, *ĝ^{h}, *g^{wh}	*γ	*g	*g, *ɡ, *γ, *ɣ	*g	*:
*k-	*k	k, c	*k, *g?	*k	*g, ĝ, *g^{w}	*k	*k˺	*k, *q	*k	*k
*-k-	*k	k, c		*k	*g, ĝ, *g^{w}	*k	*g, *k	*g, *ɡ, *γ, *ɣ	*g	*k
*ḳ-	*ḳ, *k	q	*ɣ̮, *k	*ḳ	*k, k̂, *k^{w}	*k	*k‘, *k˺	*k, *q	*x	*k
*-ḳ-	*ḳ	‘?		*ɣ	*x, *x^{w}, [*x̂?]	*Ø	*Ø	*Ø	*Ø, ?*g	*Ø

3. Comments on Dolgopolsky's Treatment of Phonology and Methodology

It is not clear why Semitic, Egyptian, and Berber are given separate treatment in the above table. These are merely three branches of Afrasian. The other branches (Cushitic, Omotic, Chadic) are not listed. It is the reconstructed Proto-Afrasian phonemes that should have been compared instead.

Dolgopolsky interprets the Proto-Nostratic sounds reconstructed as *ṗ, *ṭ, *ḳ as "emphatics". This interpretation, however, is highly questionable. Emphatics of the type found in Arabic and Berber, for example, are unlikely to have yielded the reflexes in the Nostratic daughter languages proposed by Dolgopolsky (for one thing, emphatics are notoriously prone to have assimilatory effects on adjacent vowels, and no such effects are observable in the Nostratic reconstructions proposed by Dolgopolsky or, for that matter, in any of the data from the daughter languages). Far more probable is the interpretation of this series as glottalics (ejectives), as originally proposed by Illič-Svityč and supported by Dolgopolsky (1989:90) himself until recently. The evidence for such an interpretation comes from Afrasian and Kartvelian, and that evidence is fairly solid.

The entire section on phonology in Dolgopolsky's *Nostratic Dictionary* gives the impression that it was hastily thrown together. Moreover, parts are based upon outdated or questionable scholarship within each branch — the Proto-Indo-European phonological system, to cite one example, is based exclusively upon Neogrammarian views with the addition of laryngeals. Recent scholarship is entirely ignored. At least passing mention should have been made concerning the Glottalic Theory of Proto-Indo-European consonantism (see below) proposed by Thomas V. Gamkrelidze, Vjačeslav V. Ivanov, and Paul J. Hopper and why Dolgopolsky rejects their views.

The vast majority of Indo-Europeanists posit either three or four laryngeals for the Indo-European parent language, while Dolgopolsky posits a multitude of controversial phonemes here, most conveniently subsumed under cover symbols, without further explanation as to their phonetic make-up, their vowel-coloring or lengthening effects, or their development in the Indo-European daughter languages. The evidence of Afrasian plus the judicious use of linguistic typology provide useful tools for a more accurate specification of the Proto-Indo-European laryngeals and their probable development. A good model is the 1969 paper by Joseph H. Greenberg entitled "Some Methods of Dynamic Comparison in Linguistics", in which Greenberg examines the development of similar sounds in Coptic and then draws upon his findings to explain developments in Indo-European. This is one area where the other Nostratic languages can clarify the question of the number of laryngeals to be reconstructed, their prehistoric development within the Indo-European parent language, and their probable phonetic make-up. Dolgopolsky has missed a critical opportunity to show that the Nostratic Hypothesis can offer explanations that are not available on the basis of Indo-European data alone. There are many other such missed opportunities from the other Nostratic daughter languages as well. It is just this sort of thing, namely, the ability to offer credible solutions to hitherto intractable problems within each branch, that will lend credibility to the Nostratic Hypothesis.

There is still no consensus concerning major parts of the reconstruction of the Proto-Afrasian consonant system. Though some series (labials, dentals, velars, etc.) are fairly well established, the sibilants, affricates, and fricative laterals, in particular, are far from being fully understood, and the reconstruction of labiovelars and postvelars is hotly contested. Thus, any assumptions made by those using Afrasian data are going to be controversial. Dolgopolsky's failure to lay out his own views here greatly diminishes the viability of the Nostratic etymologies he proposes based upon the sounds in question.

Older views of Altaic phonology (Ramstedt, Poppe, Street, etc.) held that the Proto-Altaic consonant system was characterized by a two-way contrast of voiceless (aspirated) vs. voiced members. More recent views (Illič-Svityč, Sergej Starostin, Anna Dybo, Oleg Mudrak, etc.) propose a three-way contrast of plain voiceless vs. voiceless aspirated vs. voiced members. Even though Dolgopolsky prefers to treat Mongolian, Manchu-Tungus, and Turkic (the core Altaic languages) as three independent branches of Nostratic, an explanation of the prehistoric development of their phonology is an absolute necessity, inasmuch as these languages are among the most contentious areas in Nostratic studies (not to mention Altaic studies).

In general, Dolgopolsky's methodology appears to be rather lax. This is not to say that there are not some brilliant etymologies in his *Nostratic Dictionary* — there are. However, there are simply too many unexplained violations of the sound laws, there are too many dubious reconstructed forms, and there is too wide a latitude in the semantics of many of the supporting forms from the daughter languages. Dolgopolsky even includes entries that he calls "doubtful", "highly doubtful", "questionable", "ambiguous", etc. Such entries should not have been included — they severely weaken the case. Moreover, there are far too many forms that have more than one possible Nostratic etymology. A fair number of these forms require ad hoc explanations to make them "fit in", no matter where they are placed — only the best of the best of such forms should have been included. Any endeavor to establish a higher-level linguistic taxon such as Proto-Nostratic is going to be controversial from the start. Consequently, in order to be even moderately credible, it is imperative that the highest methodological standards be observed in the choice of the material being compared, in the meanings assigned to reconstructed forms, in only assigning meanings that take into consideration the cultural, environmental, and social setting at the time that the proto-language is alleged to have been spoken, in the strict adherence to sound laws, in providing clear, convincing explanations for any exceptions to the established sound laws, in eliminating borrowings and/or *Wanderwörter*, in respecting and staying within the bounds of the established scholarship within each of the languages/language families being compared, etc. Methodological rigor will go a long way to quelling the misgivings of skeptics, while methodological laxity will only bring condemnation. And when the condemnation occurs, the positive attributes tend to get lost in the process, if they are even mentioned at all.

A major shortcoming of Dolgopolsky's work concerns his treatement of the Proto-Nostratic vowels. It is troublesome, to say the least, when there are irreconcilable differences in the supporting forms cited from those languages (Dravidian, Uralic, and Altaic) in which the vowels of the initial syllable are alleged to be particularly well preserved. In fairness, Dolgopolsky does attempt to explain exceptions to the established correspondences. However, many of his explanations are purely ad hoc. "Ad hoc" does not constitute a law. Dolgopolsky simply needs to offer better explanations when there are wide discrepancies in the forms cited, or these forms need to be abandoned.

In his effort to reconstruct the greatest number of forms possible for the Nostratic parent language, Dolgopolsky fails to identify underlying stems. For example, it is clear that all of the entries given below are related (assuming here, for the sake of argument, that each is a valid etymology in its own right) — they are all derivatives of an underlying *P̲aL[] 'to split, to divide', to which various extensions have been added:

1716. *pal̄Uü 'axe, hammer'.
1717. *P̲ä[l̦̲]ḳ 'to split lengthwise, to divide'.
1718. *pal []ṭ 'to split', 'axe'.
1720. *P̲LhE[ǯ] and/or *P̲LhE[c̣|ć̣] 'to split, to separate'.

In this case, it is the underlying stem *P̲aL[] 'to split, to divide' that should have been reconstructed as entry no. 1716. The remaining entries should then have been identified as derivatives of this stem and numbered 1716a, 1716b, 1716c, and 1716d.

4. Critique of Moscovite Views

Let me begin by stating unequivocally that I have the highest admiration for what Moscovite scholarship (especially the work of V. M. Illič-Svityč and A. B. Dolgopolsky — some of the work done by other Russian scholars is not on the same level) on Nostratic has achieved. Their research has opened up new and exciting possibilities and given Nostratic studies new respectability. However, this does not mean that I agree with everything they say. I regard their work as a pioneering effort and, as such, subject to modification in light of advances in linguistic theory, in light of new data from the Nostratic daughter languages, and in light of findings from typological studies that give us a better understanding of the kind of patterning that is found in natural languages as well as a better understanding of what is characteristic of language in general, including language change.

Now, in 1972 and 1973, the Georgian scholar Thomas V. Gamkrelidze and the Russian scholar Vjačeslav V. Ivanov jointly proposed a radical reinterpretation of the Proto-Indo-European stop system. According to their reinterpretation, the Proto-Indo-European stop system was characterized by the three-way contrast glottalized ~ voiced (aspirated) ~ voiceless (aspirated), as follows (this is taken from Gamkrelidze 1976:403; the reconstruction of the Proto-Indo-European stop system proposed by Winfred P. Lehmann [1952:99] is given for comparison):

Lehmann				Gamkrelidze(—Ivanov)		
b	b^h	p	=	p'	bh/b	ph/p
d	d^h	t	=	t'	dh/d	th/t
g	g^h	k	=	k'	gh/g	kh/k
g^w	g^{wh}	k^w	=	k'	gh/g	kh/k

In this revised interpretation, aspiration is viewed as a redundant feature, and the phonemes in question could also be realized as allophonic variants without aspiration. Paul J. Hopper made a similar proposal at about the same time (Hopper 1973). I should point out here that, even though I support the revisions proposed by Gamkrelidze, Hopper, and Ivanov, my views are not dependent upon any particular reconstruction of the Indo-European stop system — the sound correspondences I have proposed can be maintained using the traditional reconstruction as well. What the new views of Indo-European consonantism did was bring into light the implausibility of certain Nostratic sound correspondences established by Illič-Svityč and Dolgopolsky (see below for details). Moreover, this new interpretation opened new possibilities for comparing Proto-Indo-European with the other Nostratic daughter languages, especially Proto-Kartvelian and Proto-Afrasian, each of which had a similar three-way contrast. The most straightforward assumption would be that the glottalized stops posited by Gamkrelidze, Hopper, and Ivanov for Proto-Indo-European would correspond to glottalized stops in Proto-Kartvelian and Proto-Afrasian, while the voiceless stops would correspond to voiceless stops and voiced stops to voiced stops. This, however, is quite different from the correspondences proposed by Illič-Svityč and Dolgopolsky. They see the glottalized stops of Proto-Kartvelian and Proto-Afrasian as corresponding to the traditional plain voiceless stops of Proto-Indo-European, while the voiceless stops in the former two branches are seen as corresponding to the traditional plain voiced stops of Proto-Indo-European, and, finally, the voiced stops to the traditional voiced aspirates of Proto-Indo-European. Illič-Svityč and Dolgopolsky then reconstruct the Proto-Nostratic phonological system on the model of Kartvelian and Afrasian, with the three-way contrast glottalized ~ voiceless ~ voiced in the series of stops and affricates.

The mistake that Illič-Svityč and Dolgopolsky made was in trying to equate the glottalized stops of Proto-Kartvelian and Proto-Afrasian with the traditional plain voiceless stops of Proto-Indo-European. Their reconstruction would make the glottalized stops the least marked members in the Proto-Nostratic labial series and the most marked in the velar series. Such a reconstruction is thus in contradiction to typological evidence, according to which glottalized stops uniformly have the opposite frequency distribution (most marked in the labial series and least marked in the velar series [for details, cf. Gamkrelidze 1978]). The reason that Illič-Svityč's and Dolgopolsky's reconstruction contradicts the typological evidence is as follows: Illič-Svityč and Dolgopolsky posit glottalics for Proto-Nostratic on the basis of a small number of seemingly solid examples in which glottalics in Proto-Afrasian and/or Proto-Kartvelian appear to correspond to traditional plain voiceless stops in Proto-Indo-European. On the basis of these examples, they assume that, *whenever there is a voiceless stop in the Proto-Indo-European examples they cite, a glottalic is to be reconstructed for Proto-Nostratic, even when there are no glottalics in the corresponding Kartvelian and Afrasian forms*! This means that the Proto-Nostratic glottalics have the same frequency distribution as the Proto-Indo-European plain voiceless stops (Alexis Manaster Ramer 1997:94—95 makes the same observation [see below]). Clearly, this cannot be correct. The main consequence of the mistaken comparison of the glottalized stops of Proto-Kartvelian and Proto-Afrasian with the traditional plain voiceless stops of Proto-Indo-European is that Illič-Svityč and Dolgopolsky are led to posit forms for Proto-Nostratic on the basis of theoretical considerations but for which there is absolutely no evidence in any of the Nostratic daughter languages. Let us look at one or two examples to illustrate the ad hoc nature of these reconstructions:

1. Dolgopolsky (1998:17 and forthcoming, no. 2312) reconstructs a second singular personal pronoun *_ṭü_ > *_ṭi_ ‘thou’, with an initial glottalized dental, on the basis of data from Indo-European, Afrasian, Uralic, and Mongolian. When one looks at the attested forms in the daughter languages, one cannot find a single form anywhere that begins with a glottalized consonant. Indeed, in natural languages having glottalized consonants, these sounds tend to be underrepresented in pronoun stems and inflectional affixes. What, then, is the basis for the reconstruction *_ṭü_? — nothing more than an ad hoc rule set up by Illič-Svityč.
2. Dolgopolsky (1998:17 and forthcoming, no. 981) also reconstructs an interrogative stem *_ḳo-_ ‘who?’ (see also Illič-Svityč 1971— .I:355—356, no. 232, *_o_ ‘who’). As in the preceding example, there is not a shred of evidence in any of the Nostratic daughter languages to support the reconstruction of an initial glottalized velar in this stem.

Do these criticisms completely invalidate the cognate sets proposed by Illič-Svityč and Dolgopolsky in which glottalics in Kartvelian and Afrasian appear to correspond to plain voiceless stops in Indo-European? Well, no, not exactly — it is not quite that simple. In some cases, the etymologies are correct, but the Proto-Nostratic reconstructions are wrong. This applies to the examples cited above — for the second person personal pronoun, I would reconstruct Proto-Nostratic *_tʰi_, and, in place of *_ḳo-_ ‘who?’, I would reconstruct Proto-Nostratic *_kʷʰa-_. Other examples adduced by Illič-Svityč and Dolgopolsky admit alternative explanations, while still others are questionable from a semantic point of view and should be abandoned. Once the questionable examples are removed, there is an extremely small number (no more than a handful) left over that appear to support their position. However, compared to the massive counter-evidence (see Appendix for examples) in which glottalized stops in Proto-Kartvelian and Proto-Afrasian correspond to similar sounds (the traditional plain voiced stops) in Proto-Indo-European, even these residual examples become suspect (they may be borrowings or simply false cognates). Finally, there are even some examples where the comparison of glottalized stops in Proto-Kartvelian and Proto-Afrasian with plain voiceless stops in Proto-Indo-European is correct. This occurs in the cases where two glottalics originally appeared in a Proto-Nostratic root: *_C’VC’-_. Such roots are preserved without change in Proto-Kartvelian and Proto-Afrasian, while in Proto-Indo-European, they have been subject to a rule of regressive deglottalization: *_C’VC’-_ > *_CVC’-_.

Another major problem area is Illič-Svityč’s reconstruction of the Proto-Nostratic vowel system, which, according to him, is essentially that of modern Finnish. It simply stretches credibility beyond reasonable bounds to assume that the Proto-Nostratic vowel system could have been preserved unchanged in Finnish, especially considering the many millennia that must have passed between the dissolution of the Nostratic parent language and the emergence of Finnish. No doubt, this erroneous reconstruction came about as a result of Illič-Svityč’s failure to deal with the question of subgrouping. The Uralic phylum, of which Finnish is a member, belongs to the Eurasiatic branch of Nostratic. Now, Eurasiatic (ca. 9000 BCE) is several millennia younger than Afrasian (ca. 12000 BCE), which appears to be the oldest branch of the Nostratic macrofamily. Therefore, Proto-Afrasian must play a key role in the reconstruction of the Proto-Nostratic vowel system, and the Proto-Uralic (ca. 4000 BCE) vowel system must be considered a later development that cannot possibly represent the original state of affairs.

Dolgopolsky's reconstruction of the Proto-Nostratic vowels, on the other hand, appears to be an improvement over that proposed by Illič-Svityč (but note the comments at the end of §3 above about Dolgopolsky's treatment of the vowels), except for **ä* and **ü*, which are highly speculative. As noted by Dolgopolsky, the Proto-Nostratic vowels were at least partially preserved in initial syllables in Uralic, Dravidian, and Altaic. However, they appear to have been originally preserved in Proto-Afrasian as well. Within Afrasian, Cushitic and Omotic are particularly conservative in their vocalism, while the vowel systems found in Semitic, Egyptian, and Berber exhibit a wholesale reduction of the inherited system. Thus, notions of what Proto-Afrasian vocalism might have been like based upon the Semitic model are likely to be wrong. It turns out that Afrasian vocalism was highly archaic and, consequently, has an important role to play in the reconstruction of Proto-Nostratic vocalism.

The system of ablaut found in Semitic, Egyptian, and Berber, it may be noted, initially arose through morphological processes. It appeared quite early in verbal stems and derivative nominal stems, though primary root nouns continued to maintain stable vocalism right up to the emergence of the individual daughter languages. Once established, the system of ablaut was greatly expanded, especially in Semitic.

The inherited vowel system underwent a thorough restructuring in both Proto-Indo-European and Proto-Kartvelian as a result of a complicated series of changes initiated by the phonemicization of a strong stress accent in the early prehistory of these branches. As pointed out by Dolgopolsky, these developments diminish the importance of Kartvelian and Indo-European for ascertaining the Proto-Nostratic vowel system.

5. Basis for the Glottalic Reinterpretation of Proto-Indo-European Consonantism

At the beginning of the 20th century, the Neogrammarian reconstruction of the Proto-Indo-European phonological system was widely accepted as being an accurate representation of what was thought to have existed in the Indo-European parent language, at least in the latest stages of its development. The Neogrammarian reconstruction, which was arrived at through strict adherence to the principle that sound laws admit no exceptions, was notable for its large inventory of stops and its extremely small inventory of fricatives. The stop system consists of a four-way contrast of (A) plain voiceless stops ~ (B) voiceless aspirated stops ~ (C) plain voiced stops ~ (D) voiced aspirated stops. This system is extremely close to the phonological system of Old Indic. Actually, there were two competing versions of the Proto-Indo-European phonological system at this time: (A) the German system (cf. Brugmann 1904:52), which was phonetically based, and (B) the French system (cf. Meillet 1964:82—145), which was phonologically based.

It must be pointed out that, in spite of its wide acceptance, a small group of scholars has, from time to time, questioned the validity of the Neogrammarian reconstruction, at least in part.

Brugmann's (1904:52) reconstruction is as follows:

Monophthongs:		e	o	a	i	u	ə	
		ē	ō	ā	ī	ū		
Diphthongs:	ei̯	oi̯	ai̯	əi̯	eu̯	ou̯	au̯	əu̯
	ēi̯	ōi̯	āi̯		ēu̯	ōu̯	āu̯	
Semivowels:		i̯	u̯	(j ?)				
Liquids and Nasals:			l	r	m	n	n̂	n̲
Syllabic Liquids and Nasals:			l̥	r̥	m̥	n̥	n̥̂	ŋ̥
			l̥̄	r̥̄	m̥̄	n̥̄	n̥̂̄	ŋ̥̄
Occlusives:	p	ph	b	bh	(labial)			
	t	th	d	dh	(dental)			
	k̂	k̂h	ĝ	ĝh	(palatal)			
	q	qh	ʒ	ʒh	(pure velar)			
	q	qh	ʒ	ʒh	(labiovelar)			
Spirants:	s	sh	z	zh	þ	þh	ð	ðh

Brugmann reconstructed five short vowels and five long vowels plus a reduced vowel, the so-called "schwa indogermanicum" (also called "schwa primum"), written **ə*, which alternated with so-called "original" long vowels. A full set of diphthongs was posited as well. Finally, the system contained the semivowels **i̯* and **u̯*, a series of plain and aspirated spirants, several nasals, and the liquids **l* and **r*. The nasals and liquids were unique in their ability to function as syllabics or nonsyllabics, depending upon their environment. They were nonsyllabic (A) when between vowels or initially before vowels, (B) when preceded by a vowel and followed by a consonant, and (C) when preceded by a consonant and followed by a vowel. The syllabic forms arose in early Proto-Indo-European when the stress-conditioned loss of former contiguous vowels left them between two nonsyllabics.

It should be noted here that the Proto-Indo-European vowels were subject to various alternations that were partially correlated with the positioning of the accent within a word. These vowel alternations served to indicate different types of grammatical formations. The most common alternation was the interchange between the vowels **e* and **o* in a given syllable. There was also an alternation among lengthened-grade vowels, normal-grade vowels, and reduced-grade and/or zero-grade vowels (for details, cf. Brugmann 1904:138—150; Fortson 2004:73—76).

Meillet's reconstruction differs from that of Brugmann in several important respects. First, Meillet (1964:91—95) reconstructs only two guttural series, namely, palatals and labiovelars — he does not recognize a separate pure velar series.

Brugmann posited a separate series of voiceless aspirates for Proto-Indo-European on the basis of an extremely small, and somewhat controversial, set of correspondences from Indo-Iranian, Armenian, and Greek. In the other daughter languages, the voiceless aspirates and plain voiceless stops have the same treatment, except that **kh* appears to have became *x* in a small number of examples in Slavic — however, these examples are better explained as borrowings from Iranian rather than as due to regular developments in Slavic. As early as 1891, in a paper read before the Société de Linguistique de Paris, the Swiss scholar Ferdinand de Saussure suggested that the voiceless aspirates might have had a secondary origin, arising from earlier clusters of plain voiceless stop plus a following "coefficient sonantique". This idea was taken up by Meillet (1964:90—91), who pointed out the great rarity of the voiceless aspirates, noting in particular that the dental voiceless aspirate **th* often appears to be the result of aspiration of a plain voiceless dental by a following **ə*: **t* + **ə* > **th*, at least in Sanskrit. Current thinking on the part of a great many linguists is that the series of voiceless aspirates reconstructed by Brugmann for the Indo-European parent language should be removed, being secondarily derived in the individual daughter languages. The main opponent of this view is Oswald Szemerényi, who has argued for the reinstatement of the voiceless aspirates and, accordingly, for a return to Brugmann's four-stop system (plain voiceless ~ voiceless aspirated ~ plain voiced ~ voiced aspirated).

Particularly noteworthy is Meillet's (1964:105—126) treatment of the resonants. Here, he considers **i* and **u* to be the syllabic allophones of **y* (Brugmann's **i̯*) and **w* (Brugmann's **u̯*) respectively and classes them with the resonants, thus: **i/*y*, **u/*w*, **m̥/*m*, **n̥/*n*, **r̥/*r*, **l̥/*l*, that is to say that he does not consider **i* and **u* to be independent phonemic entities. The diphthongs are analyzed by Meillet as clusters of (A) vowel plus nonsyllabic resonant and (B) nonsyllabic resonant plus vowel.

Meillet's (1964:82—145) reconstruction may be represented as follows:

Vowels:	e	o	a				
	ē	ō	ā				
Resonants:	i/y	u/w	m̥/m	n̥/n	r̥/r	l̥/l	ə
Occlusives:	p	ph	b	bh	(labial)		
	t	th	d	dh	(dental)		
	k_1	k_1h	g_1	g_1h	(palatal)		
	k^w	k^wh	g^w	g^wh	(labiovelar)		
Sibilant:	s						

In 1878, the young Ferdinand de Saussure attempted to show that so-called "original" long vowels were to be derived from earlier sequences of short vowel plus a following "coefficient sonantique". In 1927, Jerzy Kury³owicz demonstrated that reflexes of de Saussure's "coefficients sonantiques" were preserved in Hittite. On this basis, a series of consonantal phonemes, commonly called "laryngeals", was then posited for Proto-Indo-European. Kury³owicz, in particular, set up four laryngeals, which he writes *ə̯$_1$, *ə̯$_2$, *ə̯$_3$, *ə̯$_4$. The overwhelming majority of scholars currently accept some form of this theory, though there is still no general agreement on the number of laryngeals to be reconstructed for Proto-Indo-European or on their probable phonetic values. On the basis of comparison with other Nostratic languages as well as internal considerations within Indo-European, the following phonetic values may be assigned to the laryngeals (for details on my views on the laryngeals, cf. Bomhard 2004):

*ə̯$_1$	=	Glottal stop /ʔ/
*ə̯$_2$	=	Voiceless and voiced multiply-articulated pharyngeal/laryngeal fricatives /ħ͜h/ and /ʕ͜ɦ/
*ə̯$_3$	=	Voiceless and voiced multiply-articulated pharyngeal/laryngeal fricatives /ħ͜h/ and /ʕ͜ɦ/
*ə̯$_4$	=	Voiceless glottal fricative /h/

With the reduction of the gutturals to two series, the removal of the traditional voiceless aspirates, the reanalysis of the diphthongs as clusters of vowel plus nonsyllabic resonant and nonsyllabic resonant plus vowel, and the addition of laryngeals, we arrive at the system of Lehmann (1952:99):

1. Obstruents: p t k k^w
b d g g^w
b^h d^h g^h g^{wh}
s
2. Resonants: m n
w r l y
3. Vowels: e a o e
i· e· a· o· u·
4. Laryngeals: x γ h ʔ

Now, the removal of the traditional voiceless aspirates creates a problem from a typological point of view. Data collected from the study of a great number of the world's languages have failed to turn up any systems in which voiced aspirates are added to the pair plain voiceless stop ~ plain voiced stop unless there are also corresponding voiceless aspirated stops in the system (cf. Jakobson 1971[1957]:528). This is an important point, affecting the entire structure of the traditional reconstruction. In order to rectify this imbalance, several scholars have sought typological parallels with systems such as those found, for example, in Javanese. In these rare systems, there is a three-way contrast, sometimes described as (A) plain (unaspirated) voiceless ~ (B) voiced ~ (C) "voiced aspirated": /T/ ~ /D/ ~ /D^h/. However, this interpretation is based upon a lack of understanding of the phonetics involved. Series (C) in such systems is, in reality, voiceless with breathy release — something like /$t^ɦ$/ — and not "voiced aspirated" (cf. Maddieson 1984:207).

As we have seen from the preceding discussion, Lehmann's reconstruction is problematical from a typological point of view. However, from a structural point of view, it presents an accurate analysis of Proto-Indo-European phonological patterning.

Several scholars have proposed various solutions in an attempt to eliminate the problems caused by the removal of the traditional voiceless aspirates. For example, Jerzy Kury³owicz (1964:13) tried to show that the voiced aspirates were not phonemically voiced. However, this interpretation seems unlikely in view of the fact that the daughter languages are nearly unanimous in pointing to some sort of voicing in this series in the Indo-European parent language (for correspondences and examples, cf. Meillet 1964:86—88). The main exceptions are Tocharian and possibly Hittite (at least according to some scholars). In each case, however, it is known that the voicing contrast was eliminated and that the reflexes found in these daughter languages do not represent the original state. The Greek and Italic developments are a little more complicated: in these daughter languages, the traditional voiced aspirates were devoiced, thus becoming voiceless aspirates. Then, in Italic, the resulting voiceless aspirates became voiceless fricatives:

$$b^h, d^h, g^h, g^{wh} \quad > \quad p^h, t^h, k^h, k^{wh} \quad > \quad f, \theta, \chi, \chi w$$

According to Eduard Prokosch (1938:39—41), on the other hand, the voiced aspirates of traditional grammar were really the voiceless fricatives *φ, *θ, *χ, *χw (= *bh, *dh, *gh, *g^wh respectively). This interpretation seems unlikely for two reasons: (A) as noted above, the daughter languages point to voicing in this series in Proto-Indo-European, and (B) the daughter languages point to stops as the original mode of articulation and not fricatives. This latter objection may also be raised against the theory — advocated by Alois Walde (1897:491) and Johann Knobloch (1965:163) — that the voiced aspirates may have been the voiced fricatives *β, *$ð$, *γ, *γw (= *bh, *dh, *gh, *g^wh respectively).

Next, there is the theory put forth by Louis Hammerich (1967:839—849) that the voiced aspirates may have been emphatics. Hammerich does not define what he means by the term "emphatics" but implies that they are to be equated with the emphatics of Semitic grammar. Now, in Arabic, the emphatics have been described as either uvularized or pharyngealized. Such sounds are always accompanied by backing of adjacent vowels. In Proto-Indo-European, all vowels were found in the neighborhood of the voiced aspirates, and there is no indication that any of these sounds had different allophones here than when contiguous with other sounds. Had the voiced aspirates been emphatics such as those found in Arabic, they would have caused backing of contiguous vowels, and this would be reflected in the daughter languages in some manner. However, this is not the case. If, on the other hand, the emphatics had been ejectives such as those found in the Modern South Arabian languages, the Semitic languages of Ethiopia, and several Eastern Neo-Aramaic dialects (such as, for instance, Urmian Nestorian Neo-Aramaic and Kurdistani Jewish Neo-Aramaic), the question arises as to how these sounds could have developed into the voiced aspirates needed to explain the developments in Indo-Iranian, Greek, Italic, and Armenian.

Oswald Szemerényi (1967:65—99) was one of the first to bring typological data to bear on the problem of reconstructing the Proto-Indo-European phonological system. Taking note of Jakobson's (1971[1957]:528) remark that:

> ...no language adds to the pair */t/* ~ */d/* a voiced aspirate */dʰ/* without having its voiceless counterpart */tʰ/*...

Szemerényi reasoned that since Proto-Indo-European had voiced aspirates, it must also have had voiceless aspirates. Though on the surface this reasoning appears sound, it puts too much emphasis on the typological data and too little on the data from the Indo-European daughter languages. As mentioned above, there are very cogent reasons for removing the traditional voiceless aspirates from Proto-Indo-European, and these reasons are not easily dismissed. Szemerényi also tried to show that Proto-Indo-European had only one laryngeal, namely, the voiceless glottal fricative /h/. Szemerényi's (1967:96—97) reconstruction is as follows:

	p	t	k′	k	kʷ	
	pʰ	tʰ	k′ʰ	kʰ	kʷʰ	
	b	d	g′	g	gʷ	
	bʰ	dʰ	g′ʰ	gʰ	gʷʰ	
			y	w		
		l	r	m	n	
			s	h		
	a	e	o	i	u	ə
	ā	ē	ō	ī	ū	
(also the sequences	ah	eh	oh	ih	uh)	

Szemerényi does not include diphthongs in his reconstruction since their "phonemic status is disputed".

Szemerényi's reconstruction is in fact typologically natural, and he defended it strongly right up through his last major work (cf. Szemerényi 1996:37—70). His system — as well as that of the Neogrammarians, it may be added — is merely a projection backward in time of the Old Indic phonological system. In certain dialects of "Disintegrating Indo-European" (specifically, in the early development of Pre-Indo-Iranian, Pre-Greek, and Pre-Italic), such a system no doubt existed in point of fact.

Next, there are the proposals put forth by Joseph Emonds (1972). According to Emonds, the plain voiced stops of traditional Proto-Indo-European are to be reinterpreted as plain lax voiceless stops, while the traditional plain voiceless stops are taken to have been tense and aspirated:

Lehmann					Emonds			
p	t	k	k^{w}	=	ph	th	kh	kh^{w}
b	d	g	g^{w}	=	p	t	k	k^{w}
b^{h}	d^{h}	g^{h}	g^{wh}	=	bh	dh	gh	gh^{w}

Emonds regards the voicing of the lax stops as common to a Central innovating area and the appearance of voiceless stops in Germanic, Armenian, and Hittite as relics.

Similar proposals were put forth by Toby D. Griffen (1988:162—189). According to Griffen, Proto-Indo-European had a three-member stop system, which he represents as (using the dentals for illustration) *[d], *[t], *[t^{h}] (media, tenuis, aspirata). While this system was maintained in Germanic with only minor changes, a series of sound-shifts in the other Indo-European daughter languages completely restructured the inherited system. Thus, Germanic emerges as the most conservative daughter language in its treatment of the Proto-Indo-European stop system.

There are other problems with the traditional reconstruction besides the typological difficulties caused by the removal of the voiceless aspirates. Another problem, noted in most of the standard handbooks, is the statistically low frequency of occurrence — perhaps total absence — of the traditional voiced labial stop **b*. We may cite Meillet's (1964:89) comments on this matter:

> *b* is relatively rare; it does not occur in any important suffix nor in any ending; it is secondary in some of the words where it is found, thus, Skt. *píbāmi* "I drink", OIr. *ibim* "I drink", Lat. *bibō* (with initial *b* through assimilation) is an ancient reduplicated form in view of Skt. *pāhi* "drink", Gk. πῖθι, OCS. *piti* "to drink", Lat. *pōculum* "cup"; ...other words are imitative, thus Gk. βάρβαρος, Lat. *balbus*, etc.; still others are limited to a few languages and give the impression of being recent borrowings.

The marginal status of **b* is difficult to understand from a typological viewpoint and is totally unexplainable within the traditional framework. This problem was investigated in 1951 by the Danish scholar Holger Pedersen. Pedersen noted that, in natural languages having a voicing contrast in stops, if there is a missing member in the labial series, it is /p/ that is missing and not /b/. This observation led Pedersen to suggest that the traditional plain voiced stops might originally have been plain voiceless stops, while the traditional plain voiceless stops might have been plain voiced stops:

Brugmann						Pedersen				
b	d	ĝ	ʒ	ʒ	=	Ø	t	k̂	k	k^{w}
p	t	k̂	q	q	=	b	d	ĝ	g	g^{w}

Later shifts would have changed the earlier plain voiced stops into the traditional plain voiceless stops and the earlier plain voiceless stops into the traditional plain voiced stops. In a footnote in his 1953 *BSL* article entitled "Remarques sur le consonantisme sémitique", André Martinet (1975[1953]:251—252, fn. 1) objected to this "musical chairs" rearrangement:

> Since there are extremely few examples of the Common Indo-European phoneme reconstructed "analogically" as **b*, it is tempting to diagnose a gap there as well, as the late Holger Pedersen did in *Die gemeinindoeuropäischen und die vorindoeuropäischen Verschlusslaute*, pp. 10-16. But, instead of assuming, as did Pedersen, the loss of a Pre-Indo-European **p* followed by a musical-chairs [rearrangement] of *mediae* and *tenues*, one should be able to see in the series **d*, **g*, **g*^{w} the result of evolution from an earlier series of glottalics, without labial representative.

This appears to be the first time that anyone had proposed reinterpreting the plain voiced stops of traditional Proto-Indo-European as glottalics. Martinet's observation, however, seems to have influenced neither Gamkrelidze and Ivanov nor Hopper, each of whom arrived at the same conclusion independently of Martinet as well as independently of each other.

Discovery — perhaps "rediscovery" would be a better term since Martinet's insightful remarks first appeared in 1953 — of what has come to be known as the "Glottalic Theory" came from two separate sources, each working independently. On the one-hand, the British-born American Germanist Paul J. Hopper hit upon the notion that Proto-Indo-European may have had a series of glottalized stops while he was a student at the University of Texas and taking a course in Kabardian from Aert Kuipers. Hopper went on about other business after graduation, waiting five years before putting his ideas into writing. On the other hand, the Georgian Indo-Europeanist Thomas V. Gamkrelidze, a native speaker of a language containing glottalics (Georgian), had been investigating the typological similarities between Proto-Kartvelian and Proto-Indo-European (cf. Gamkrelidze 1966 and 1967). It did not take Gamkrelidze long to realize the possibility that Proto-Indo-European might also have had glottalized stops. Gamkrelidze, in a joint article with the Russian Indo-Europeanist Vjačeslav V. Ivanov, was the first to make it into print (Gamkrelidze—Ivanov 1972). Hopper might have beat them into print had his paper on the subject not been rejected by the journal *Language*. He was then obliged to search for another journal willing to publish his views, which finally happened in 1973. Then, in 1973, Gamkrelidze and Ivanov published a German language version of their 1972 paper.

Hopper (1973:141—166) proposed reinterpreting the plain voiced stops of traditional Proto-Indo-European — Lehmann's **b*, **d*, **g*, **g*^{w} — as glottalized stops (ejectives), that is, (**p'*), **t'*, **k'*, **k'*^{w} respectively, because the traditional plain voiced stops

> show many of the typological characteristics of glottalized stops (ejectives), e.g. they are excluded from inflectional affixes, they may not cooccur with another in the same root, etc.

Hopper also reinterpreted the traditional voiced aspirates as murmured stops.

Gamkrelidze—Ivanov (1972:15—18 and 1973:150—156) also reinterpret the traditional plain voiced stops as ejectives, but, unlike Hopper, they reinterpret the traditional plain voiceless stops as voiceless aspirates. They make no changes to the traditional voiced aspirates. They point out, however, that the feature of aspiration is phonemically irrelevant in a system of this type.

Many of the points discussed above by Gamkrelidze were also noted by Hopper, in particular the root structure constraint laws (cf. Hopper 1973:158—161). Hopper also discusses possible trajectories of the new system in various Indo-European daughter languages.

The system of Gamkrelidze, Hopper, and Ivanov has several clear advantages over the traditional reconstruction of the Proto-Indo-European stop system:

1. Their reinterpretation of the traditional plain voiced stops as glottalics (ejectives) makes it easy to account for the fact that the phoneme traditionally reconstructed as **b* was highly marked in the system, being characterized by an extremely low frequency of occurrence (if it even existed at all). Such a low frequency distribution is extremely uncharacteristic of the patterning of the voiced labial stop /b/ in natural languages having a voicing contrast in stops, but it is fully characteristic of the patterning of the labial ejective /p'/ (cf. Gamkrelidze 1981:605—606; Greenberg 1970:127).
2. Not only does the reinterpretation of the traditional voiced stops as ejectives easily account for the frequency distribution of these sounds, it also explains the fact that they were used only very infrequently in inflectional affixes and pronouns, since this type of patterning is characteristic of the way ejectives behave in natural languages having such sounds.
3. For the first time, the root structure constraint laws can be credibly explained. These constraints turn out to be a simple voicing agreement rule with the corollary that two glottalics cannot cooccur in a root. Hopper (1973:160) cites Hausa, Yucatec Mayan, and Quechua as examples of natural languages exhibiting a similar constraint against the cooccurrence of two glottalics. Akkadian may be added to this list as well if we take Geers' Law to be a manifestation of such a constraint.
4. The so-called Germanic and Armenian "consonant shifts" (in German, "Lautverschiebungen"), which can only be accounted for very awkwardly within the traditional framework (cf. Emonds 1972:108—122), turn out to be mirages. Under the revised reconstruction, these branches (along with the poorly-attested Phrygian as well) turn out to be relic areas.

In 1984, Gamkrelidze and Ivanov published their monumental joint monograph entitled *Индоевропейский язык и индоевропейцы: Реконструкция и историко-типоло-гический анализ праязыка и протокультуры [Indo-European and the Indo-Europeans: A Reconstruction and Historical Typological Analysis of a Protolanguage and a Proto-Culture]* (an English translation of this work has since been published by Mouton de Gruyter [1995]). As is to be expected, this massive work (2 volumes, 1,328 pages) contains the most detailed discussion of the Glottalic Theory that has yet appeared (for additional information on the Glottalic Theory, see especially Salmons 1993; Vennemann [ed.] 1989; and Fallon 2002:225—288). Gamkrelidze and Ivanov's book also contains trajectories of the revised Proto-Indo-European phonological system in the various Indo-European daughter languages, original proposals concerning the morphological structure of the Indo-European parent language (they propose that, at an earlier stage of development, Proto-Indo-European was an active language [strong support for these views is expressed by Lehmann 1995 and 2002, among others]), an exhaustive treatment of the Proto-Indo-European lexicon, and a new theory about the homeland of the Indo-Europeans (they argue that the Indo-European homeland was located in eastern Anatolia in the vicinity of Lake Van). One of the most novel proposals put forth in the book is that Proto-Indo-European may have had labialized dentals and a labialized sibilant. Gamkrelidze—Ivanov also posit postvelars for Proto-Indo-European. Their complete reconstruction is as follows (cf. Gamkrelidze—Ivanov 1984.I:134 and 1995.I:116):

	I.	II.	III.									
1.	(p’)	b[^{h}]	p[^{h}]									
2.	t’	d[^{h}]	t[^{h}]				t’°	d[^{h}]°	t[^{h}]°			
3.	k’	g[^{h}]	k[^{h}]	k̂’	ĝ[^{h}]	k̂[^{h}]	k’°	g[^{h}]°	k[^{h}]°	s	ŝ	ś°
4.	q’	-	q[^{h}]									

Note: The consonants enclosed in the box are considered to be the most reliably reconstructed.

It is not surprising that the new look of Proto-Indo-European consonantism proposed by Gamkrelidze—Ivanov has a distinctly Caucasian appearance about it.

Though the Glottalic Theory has attracted a good deal of attention over the past three decades and has gained widespread acceptance, it should be noted that there is still some disagreement about the make-up of the traditional voiceless stops and voiced aspirates. Hopper (1973:141—166), for example, reinterprets the traditional voiced aspirates as murmured stops, making no changes to the traditional plain voiceless stops. His system is as follows:

Lehmann					Hopper			
p	t	k	k^w	=	p	t	k	k^w
b	d	g	g^w	=	p’	t’	k’	k’^{w}
b^h	d^h	g^h	g^{wh}	=	$\underline{b}$	$\underline{d}$	$\underline{g}$	$\underline{g}^w$

This differs from the views of Gamkrelidze—Ivanov, who, as noted above, regard the traditional plain voiceless stops as voiceless aspirates, while making no changes to the traditional voiced aspirates. Moreover, they consider the feature of aspiration to phonemically irrelevant, with the choice between the aspirated and nonaspirated variants being mechanically determined by the paradigmatic alternations of root morphemes.

In his most recent work, Lehmann (2002:198—202, 211—214) accepts a form of the Glottalic Theory. Lehmann (2002:200) reinterprets $*b$, $*d$, $*g$, $*g^w$ of traditional Indo-European as $*'p$, $*'t$, $*'k$, $*'k^w$ respectively, with preglottalization. Furthermore, he (2002:200) reinterprets the traditional plain voiceless stops and voiced aspirates as voiceless and voiced respectively with aspirated and unaspirated allophones. As in his earlier work (1952:100—102), Lehmann (2002:214—216) posits only palatovelars and labiovelars, assuming a secondary status for the plain velars reconstructed by the Neogrammarians. Lehmann reconstructs the following four laryngeals: *ʔ*, *h*, $*\chi$, $*\gamma$. Lehmann (2002:201) assumes that $*\chi$ and $*\gamma$ were voiceless and voiced velar fricatives respectively and that $*\gamma$ may have had a *w*-offglide. Lehmann's revised system is as follows (2002:201):

Vowels

ī ū
e ē ə o ō
a ā

Consonants

	Obstruents			Resonants				Fricatives		
Labials:	p	p'	b^h	m			w			
Dentals:	t	t'	d^h	n	r	l	y		s	
Palato-velars:	k	k'	g^h							
Labio-velars:	k^w	k'^w	g^{wh}							
Laryngeals:	ʔ							χ	γ	h

My own view is that it is necessary to recognize several distinct stages of development within Proto-Indo-European and that the traditional voiced aspirates were a relatively late development — in fact, it is probably only necessary to reconstruct them in the Disintegrating Indo-European ancestors of Indo-Iranian, Armenian, Greek, and Italic. The voiceless aspirates (the traditional plain voiceless stops), on the other hand, seem to be fairly ancient and were most likely inherited by Proto-Indo-European from Proto-Nostratic.

For the latest period of development (the stage that I have called "Disintegrating Indo-European"), I would reconstruct the Proto-Indo-European phonological system as follows:

Obstruents:	p^h	t^h	k^h	k^{wh}
	b^h	d^h	g^h	g^{wh}
	(p')	t'	k'	k'^w
		s		

Laryngeals:	ʔ	h	ħ͜h			
			ʕ͜ɦ			
Resonants:	m/m̥	n/n̥	l/l̥	r/r̥	w/u	y/i
Vowels:	e	o	a	(i)	(u)	ə
	ē	ō	ā	ī	ū	

The Glottalic Theory has not escaped criticism. One of the sharpest criticisms concerns the alleged implausibility of the changes that would be required to arrive at the plain voiced stops found in the majority of the daughter languages. This issue has been dealt with at length by Paul D. Fallon in Chapter 6, Ejective Voicing, of his 1992 book *The Synchronic and Diachronic Phonology of Ejectives.* Here, Fallon provides empirical support for the Glottalic Theory of Proto-Indo-European consonantism. After presenting and discussing in great detail evidence from a number of languages, Fallon (2002:278—285, §6.7), examines and evaluates the plausibility of various paths for ejective voicing, as follows:

1. Direct Voicing: Fallon describes the process of direct voicing of ejectives as the spread of [voice] from a vowel, "a rather direct change which telescopes what historically may have been a series of minute changes. The results will often be a change to a pulmonic voiced consonant with loss of glottal constriction..." On the other hand, "we can express this as indirect voicing in two parts, as the delinking of the laryngeal feature [c.g.], followed by default fill-in (or spreading)."
2. Indirect Voicing: "The indirect voicing of ejectives involves their loss of distinct glottalization and the subsequent voicing of the voiceless unaspirated series." This is the scenario I prefer, and which I have followed in Chapter 5 of my forthcoming book *Reconstructing Proto-Nostratic.*
3. Laryngealization: "Another commonly posited path of development from ejective to voiced is via laryngealization."
4. Implosivization: "Many linguists now believe that PIE ejectives became implosive." As an example, a little later on, Fallow suggests that, within the Quichean languages, ejectives may have become implosives as follows:

   Voiceless ejective > voiceless implosive > voiced implosive

   At a later date, the implosives would have been changed to plain voiced stops.

Fallon (2002:285) summarizes his findings by noting:

> In sum, we have seen that there is a tremendous amount of variation in the production of ejectives, both cross-linguistically and individually. I have discussed four possible directions of change from ejective to voiced: direct and indirect voicing, laryngealization, and implosivization... Creaky or laryngealized voicing seems to be

> fairly common, as we have seen in Kabardian, for example. And implosivization has occurred independently in a number of African and Central American languages. I feel that these changes are valid possibilities, and that given dialectal variation, they both could be paths of ejective development. And I hope that I have shown that we should not … automatically rule out the possibility of direct phonetic or phonological change.

And further (2002:288):

> … I also hope that I have dispelled the myth of implausibility of ejective voicing. The data gathered here do not by any means validate the Ejective Model — such validation will require careful study and reassessment of almost 200 years of assumptions (such as the papers in Vennemann 1989). However, they do help rebut some of the Glottalic Theory's sharpest criticisms and should breathe new life into the debate. Garrett (1991: 803) said the Glottalic Theory "was an exciting proposal...one whose time has come and gone". But like Mark Twain, I think rumors of its death are greatly exaggerated.

6. Revised Nostratic Sound Correspondences

Now that we have reviewed and critiqued Moscovite views on Nostratic sound correspondences and discussed the reinterpretation of Proto-Indo-European consonantism in view of the Glottalic Theory, we are in a position to investigate the implications of this hypothesis for Nostratic sound correspondences. Immediately, new possibilities suggest themselves, the most important of which is that the glottalics now reconstructed for Proto-Indo-European might correspond to similar sounds in Proto-Afrasian and Proto-Kartvelian. Indeed, a thorough examination of the reconstructed lexicons of these three proto-languages has turned up a massive number of examples in which glottalics in Proto-Indo-European correspond to glottalics in Proto-Afrasian and Proto-Kartvelian. Moreover, an equally thorough examination has turned up even more examples in which the voiceless stops reconstructed for Proto-Indo-European correspond to similar sounds in Proto-Afrasian and Proto-Kartvelian. Consequently, we are now in a position to confirm that the correct correspondences are as follows:

Proto-Nostratic	Proto-IE	Proto-Kartvelian	Proto-Afrasian	Proto-Uralic	Proto-Dravidian	Proto-Altaic	Proto-Eskimo
b-	b^{h}-	b-	b-	p-	p-	b-	p-
-b-	-b^{h}-	-b-	-b-	-w-	-pp-/-vv-	-b-	-v-
p^{h}-	p^{h}-	p-	p-, f-	p-	p-	p^{h}-	p-
-p^{h}-	-p^{h}-	-p-	-p-, -f-	-p-	-pp-/-v-	-p^{h}-	-p(p)-
p'-	(p'-)	p'-	p'-			p-	
-p'-	(-p'-)	-p'-	-p'-			-p-	
d-	d^{h}-	d-	d-	t-	t-	d-	t-
-d-	-d^{h}-	-d-	-d-	-t-	-ṭ(ṭ)-	-d-	-ð-
t^{h}-	t^{h}-	t-	t-	t-	t-	t^{h}-	t-
-t^{h}-	-t^{h}-	-t-	-t-	-t(t)-	-tt-	-t^{h}-	-t(t)-
t'-	t'-	t'-	t'-	t-	t-	t-	t-

-t'-	-t'-	-t'-	-t'-	-t-	-t(t)-	-t-	-t-
g-	g^{h}-	g-	g-	k-	k-	g-	k- q-
-g-	-g^{h}-	-g-	-g-	-x-	-k-	-g-	-ɣ-
k^{h}-	k^{h}-	k-	k-	k-	k-	k^{h}-	k- q-
-k^{h}-	-k^{h}-	-k-	-k-	-k(k)-	-k(k)-	-k^{h}-	-k(k)- -q(q)-
k'-	k'-	k'-	k'-	k-	k-	k-	k- q-
-k'-	-k'-	-k'-	-k'-	-k-	-k(k)-	-k-	-k- -q-

Not only do the revised correspondences overcome all of the objections raised above concerning Moscovite views, they are more straightforward and do not require setting up ad hoc rules to explain exceptions. Inasmuch as they are more straightforward, they satisfy the principle known as Occam's (Ockham's) Razor. *Webster's New Collegiate Dictionary* defines this principle as:

> a scientific and philosophic rule that entities should not be multiplied unnecessarily, which is interpreted as requiring that the simplest of competing theories be preferred to the more complex or that explanations of unknown phenomena be sought first in terms of known quantities.

By way of example, we may now take another look at the examples cited above for 'thou' and 'who?' to see how they are changed:

Example 1: Proto-Nostratic **t^{h}i-* (~ **t^{h}e-*) second person pronoun stem: 'thou/you':

A. Proto-Afrasian **t[i]* 'you': Proto-Semitic (prefix forms) **ti-*/**ta-*, (suffix forms) ***-tī̆*/*-*tā̆*** 'you' > Arabic (m.) *ˀan-ta*, (f.) *ˀan-ti* 'you', perfect 2nd sg. endings (m.) *-ta*, (f.) *-ti*, imperfect 2nd sg./du./pl. prefix *ta-*; Akkadian (m.) *an-tā*, (f.) *an-tī* 'you', permansive 2nd sg. endings (m.) *-āt(a)*, (f.) *-āti*, prefix conjugation 2nd sg./pl. prefix *ta-*; Hebrew (m.) *ˀat-tāh*, (f.) *ˀat-t(ī)* 'you', perfect 2nd sg. endings (m.) *-tā*, (f.) *-t(i)*, imperfect 2nd sg./pl. prefix *ti-*; Ugaritic ***ảt*** 'you' (m. **ˀatta*, f. **ˀatti*), perfect 2nd sg. ending *-t* (m. **-ta*, f. **-ti*), imperfect 2nd sg./du./pl. prefix *t-*; Śḥeri / Jibbāli *tɛn* 'you'; Geez / Ethiopic (m.) *ˀan-ta*, (f.) *ˀan-tī* 'you', prefix conjugation 2nd sg./pl. prefix ***tə-***. Central Cushitic: Bilin (sg.) *en-ti*, *in-ti* 'you', (pl.) *en-tin*, *in-tin*. Proto-East Cushitic (2nd sg. subj.) **ʔat-i/u* 'you' > Galla / Oromo *at-i* 'you'; Gedeo *at-i* 'you' Hadiyya *at-i* 'you'; Kambata *at-i* 'you'; Sidamo *at-e/i* 'you'; Burji *áši* 'you'; Somali *ad-i-* 'you'; Saho-Afar *at-u* 'you'; Rendille *at-i* 'you'; Bayso *at-i* 'you'; Konso *at-ti* 'you'; Gidole *at-te* 'you'. Proto-East Cushitic (2nd pl. subj.) **ʔatin-* 'you' > Saho-Afar *atin* 'you'; Burji *ašinu* 'you'; Somali *idin-* 'you'; Rendille *atin-* 'you'; Dasenech *itti(ni)* 'you'; Kambata *aˀn-aˀooti* 'you'; Tsamay *atun-i* 'you'. Proto-Highland East Cushitic (2nd sg. voc. fem.) **tee* 'you' > Gedeo (f.) *tee* 'you'; Hadiyya (f.) *ta* 'you'; Kambata (f.) *te* 'you'; Sidamo (f.) *tee* 'you'. Proto-Southern Cushitic (pl.) **ʔata-* 'you', (sg.) **ʔaata-* 'you' > Iraqw *aten* 'you'; Dahalo (pl.) *ˀàt̠t̠a* 'you', (sg.) *ˀáát̠a* 'you'.

B. Elamo-Dravidian: Elamite (2nd sg. verb ending) *-t*, (2nd pl. verb ending) *-ht* (*h* + *t*; in Royal Achaemenid Elamite, this becomes *-t* due to loss of *h*), allocutive (that is, person addressed or "second person") gender suffix *-t*. Dravidian: Parji *-t* appositional marker of 2nd sg. in pronominalized nouns and verb suffix of 2nd sg.

C. Proto-Indo-European (nom. sg.) *t^{h}ū̆ 'you', (acc. sg.) *t^{h}wē̆/*t^{h}ē̆, *t^{h}wēm/*t^{h}ēm, (gen. sg.) *t^{h}ewe, *t^{h}ewo, (enclitic) *t^{h}(w)ey/*t^{h}(w)oy: Sanskrit (nom. sg.) *tvám* 'you', (acc. sg.) *tvā́m*, *tvā*, (instr. sg.) *tváyā*, (dat. sg.) *túbhyam*, *te*, (abl. sg.) *tvát*, (gen. sg.) *táva*, *te*, (loc. sg.) *tváyi*; Avestan (nom. sg.) *tūm*, *tū* 'you'; Greek (Doric) (nom. sg.) τύ 'you', (gen. sg.) τέος, (dat. sg.) τοί, τοι, (acc. sg.) τέ; Armenian (nom. sg.) *du* 'you'; Albanian (nom. sg.) *ti* 'you', (dat. sg.) *ty*, *të*, (acc. sg.) *ty*, *të*, (abl. sg.) *teje*; Latin (nom. sg.) *tū* 'you', (gen. sg.) *tuī*, (dat. sg.) *tibī*, (acc. sg.) *tē*, (abl. sg.) *tē* (Old Latin *tēd*); Old Irish (nom. sg.) *tú* 'you', (gen. sg.) *taí*; Gothic (nom. sg.) *þu* 'you', (gen. sg.) *þeina*, (dat. sg.) *þus*, (acc. sg.) *þuk*; Lithuanian (nom. sg.) *tù* 'you', (acc. sg.) *tav*, (gen. sg.) *tav□s*, (loc. sg.) *tavyjè*, (dat. sg.) *táv*, (instr. sg.) *tavimì*; Old Church Slavic (nom. sg.) *ty* 'you', (acc. sg.) *tę*, *tebe*, (gen. sg.) *tebe*, (loc. sg.) *tebě*, (dat. sg.) *tebě*, *ti*, (instr. sg.) *tobojǫ*; Palaic (nom. sg.) *ti-i* 'you', (dat.-acc. sg.) *tu-ú*; Hittite (nom. sg.) *zi-ik*, *zi-ga* 'you', (acc.-dat. sg.) *tu-uk*, *tu-ga*, (gen. sg.) *tu-(e-)el*, (abl. sg.) *tu-e-da-az*, *tu-e-ta-za*; (encl. possessive nom. sg.) *-ti-iš*, (encl. possessive acc. sg.) *-ti-in*, (encl. possessive neut. sg.) *-te-it*, (encl. possessive gen. sg.) *-ta-aš*, (encl. possessive dat. sg.) *-ti*, (encl. possessive instr. sg.) *-te-it*; (encl oblique sg.) *-ta* (*-du* before *-za*); Luwian (nom. sg.) *ti-i* 'you'. Note: the Proto-Indo-European reconstructions given above represent later, post-Anatolian forms. Proto-Indo-European (2nd pl. verb ending) *-t^{h}e: Sanskrit (2nd pl. primary verb ending) *-tha*, (2nd pl. secondary verb ending) *-ta*; Greek -τε; Latin (imptv.) *-te*; Old Irish *-the*, *-de*; Gothic *-þ*; Lithuanian *-te*; Old Church Slavic *-te*.

D. Proto-Uralic (sg.) *te 'you': Finnish *sinä/sinu-* 'you'; Lapp / Saami *don/dú-* 'you'; Mordvin *ton* 'you'; Cheremis / Mari *təń* 'you'; Votyak / Udmurt *ton* 'you'; Zyrian / Komi *te* (acc. *tenõ*) 'you'; Hungarian *të* 'you'; Tavgi Samoyed / Nganasan *tannaŋ* 'you'; Yenisei Samoyed / Enets *tod'i* 'you'; Selkup Samoyed *taŋ*, *tat* 'you'; Kamassian *tan* 'you'. Yukaghir *tet* 'you'. Proto-Uralic (pl.) *te 'you': Finnish *te* 'you'; Lapp / Saami *dí* 'you'; Mordvin (Erza) *tiń*, *tïń* 'you'; Cheremis / Mari *tä*, *te* 'you'; Votyak / Udmurt *ti* 'you'; Zyrian / Komi *ti* 'you'; Hungarian *ti* 'you'; Tavgi Samoyed / Nganasan *teeŋ* 'you'; Yenisei Samoyed / Enets *tod'iʔ* 'you'; Selkup Samoyed *tee*, *tii* 'you'. Yukaghir *tit* 'you'.

E. Proto-Altaic (nom. sg.) *t^{h}i 'thou, you': Proto-Mongolian (nom. sg.) (*t^{h}i > *t^{y}i >) *či* 'you', (nom. pl.) *ta 'you' > Written Mongolian (nom. sg.) *či* 'you' (gen. *činu*), (nom. pl.) *ta*; Dagur (nom. sg.) *šī* 'you', (nom. pl.) *tā*; Monguor (nom. sg.) *ći* 'you', (nom. pl.) *ta*; Ordos (nom. sg.) *či* 'you', (nom. pl.) *ta*; Khalkha (nom.

sg.) *či* 'you', (nom. pl.) *ta*; Buriat (nom. sg.) *ši* 'you', (nom. pl.) *tā*; Moghol (nom. sg.) *či* 'you', (nom. pl.) *to*; Kalmyk (nom. sg.) *či* 'you', (nom. pl.) *ta*.

F. Etruscan: In Etruscan, there is a pronoun *θi* of unknown meaning. However, in view of the fact that the verbal imperative endings for the 2nd person are *-ti*, *-θ*, *-θi*, *θi* may be a form of the pronoun of the 2nd person singular.

G. Proto-Chukchi-Kamchatkan **tur(i)* 'you': Chukchi *turi* 'you', *turɣ-in* 'your'; Kerek (pl.) *təjəkku* 'you', (dual) *təəj* 'you', *təjəj* 'your'; Koryak (pl.) *tuju* 'you', (dual) *tuji* 'you', *tucɣ-in* 'your'; Alyutor (pl.) *turuwwi* 'you'; Kamchadal / Itelmen *tuzaˀn* 'you', *tizvin* 'your'. Proto-Chukchi-Kamchatkan **-ð* in **kəð* 'you': Chukchi *ɣət* (Southern *ɣəto*) 'you'; Kerek *hənŋu* 'you'; Koryak *ɣəcci* 'you'; Alyutor *ɣətta*, *ɣəttə* (Palana *ɣətte*) 'you'; Kamchadal / Itelmen *kəz(z)a* (Sedanka *kza*) 'you'.

H. Eskimo: West Greenlandic (2nd sg. absolutive possessive suffix) *-(i)t*.

Note that there is not a single shred of evidence from the daughter languages to support the reconstruction of an initial glottalic at the Proto-Nostratic level here, and none needs to be reconstructed when the correct sound correspondences are employed.

Example 2: Proto-Nostratic **kʷʰi-* (~ **kʷʰe-*) relative pronoun stem, **kʷʰa-* (~ **kʷʰə-*) interrogative pronoun stem:

A. Proto-Afrasian (?) **kʷa-* interrogative pronoun stem: Proto-Semitic **ka-m* 'how much?, how many?' > Arabic *kam* 'how much?, how many?'; Ḥarsūsi *kem* 'how much?, how many?; a few'; Mehri *kəm* 'how much?'; Soqoṭri *kəm* 'how much?'.

B. Proto-Indo-European **kʷʰe-/*kʷʰo-*, **kʷʰi-* stem of interrogative and relative pronouns: Sanskrit *ká-ḥ*, *kā́* 'who?', *káti* 'how many?', *kím* 'what?', *kútra* 'where?', *cid* 'even, also'; Avestan interrogative-indefinite pronoun stem *ka-* 'who', *čaiti* 'how many?'; Old Persian interrogative-indefinite pronoun stem *ka-* 'who'; Latin *quis* 'who?', *quid* 'what?', *quod* 'that, wherefore, why', *quot* 'how many?', *quisquis* 'whoever, whichever, whatever'; Greek τίς 'who?', τί 'what?', ποῦ 'where?', πόσος 'of what quantity?, how much?, how many?'; Armenian *kʰani* 'how many?'; Old Irish *cía* 'who?'; Welsh *pwy* 'who?'; Cornish *pyw* 'who?'; Breton *piou* 'who?'; Gothic *ƕas* 'who?', *ƕō* 'what?', *ƕan* 'when?', *ƕar* 'where?', *ƕarjis* 'which?', *ƕaþ* 'whereto?'; Old Icelandic *hverr* 'who?, which?, what?', *hvé* 'how?', *hvat* 'what?'; Old Swedish *ho* 'who?'; Old Danish *hwa* 'who?'; Old English *hwā* 'who?', *hwæt* 'what'; Old Frisian *hwā* 'who?'; Old Saxon *hwē*, *hwie* 'who?'; Old High German *(h)wer* 'who?' (New High German *wer*), *(h)waz* 'what?' (New High German *was*); Lithuanian *kàs* 'who?, what?', *kur̃* 'where?, whither?'; Old Church Slavic *kъto* 'who?'; Hittite interrogative pronoun (nom. sg.) *ku-iš* 'who?' (acc. *ku-in*), (neuter) *ku-it* 'what?', *ku-(u-)wa-at* 'why?', *ku-wa-(at-)tin* 'where?, whither?', *ku-wa-(a-)pí* 'where?, whither?, when?'; Palaic interrogative and relative pronoun *kuiš*; Luwian *ku-(i-)iš* 'who?', interrogative adverb *ku-wa-(a-)ti(-in)* 'how?', relative adverb *ku-wa-at-ti* 'where, whence'; Lycian interrogative and relative stem *ti*; Lydian relative pronoun *qis*; Tocharian A interrogative stem (nom.) *kus* (acc. *kuc*) 'who?, which?, what?', relative stem (nom.) *kusne* (acc. *kucne*) 'who, which', B interrogative and relative stem (nom.) k_u*se* 'who(?), whoever, no matter who; the one who, those who', (acc.) k_u*ce* 'whom?, what?, which?; whom, what, which', also used as a conjunction: 'because; (so) that'. Derivatives of this stem are abundantly represented in the Indo-European daughter languages — only a small sampling is given here.

C. Proto-Uralic **ki-* ~ **ke-* relative pronoun stem: Finnish *ken/kene-/ke-* 'who'; Estonian *kes* 'who'; Lapp / Saami *gi/gæ-* 'who, which, what'; Mordvin *ki* 'who, somebody'; Cheremis / Mari *ke*, *kö*, *kü* 'who'; Votyak / Udmurt *kin* 'who'; Zyrian / Komi *kin* 'who'; Hungarian *ki* 'who, who?'; Kamassian *giʔiʔ* 'which (of two)', *giʔgeʔ* 'what sort of', *giʔin*, *kijen*, *gin* 'where', *gildi* 'how much, how many'. Yukaghir (Southern / Kolyma) *kin* 'who', *kintek* 'who; somebody'. Proto-Uralic **ku-* ~ **ko-* interrogative pronoun stem: Finnish *kuka/ku-* 'who?', *kussa* 'where?', *koska* 'when?'; Lapp / Saami *gutti* 'who?', *gost* 'where?, from where?', *gok'tĕ* 'how?'; Mordvin *kodamo* 'which?, what kind of?', *kona* 'which?', *koso* 'where?', *koda* 'how?'; Cheremis / Mari *kudõ* 'who?, which?', *kuštõ* 'where?', *kuze* 'how?'; Votyak / Udmurt *kudiz* 'which?', *ku* 'when?'; Zyrian / Komi *kod* 'which?', *ko* 'when?'; Vogul / Mansi *hoo*, *kon* 'who?', *hoot* 'where?', *kun* 'when?'; Ostyak / Xanty *koji* 'who?', *kŏti* 'what?'; Hungarian *hol* 'where?', *hova* 'whither?', *hogy* 'how?'; Yurak Samoyed / Nenets *hu* 'who?', *huńaŋy* 'which?', *huna*, *huńana* 'where?', *hańaʔ* 'whither?'; Tavgi Samoyed / Nganasan *kua*, *kunie* 'which?', *kuninu* 'where?', *kuniʔaaŋ* 'how?'; Yenisei Samoyed / Enets *huju* 'one of two, either', *kuu* 'whither?', *kune*, *kunne* 'when?', *kunnoʔ* 'how?'; Selkup Samoyed *kutte*, *kudö* 'who?', *kun* 'where?, from where?', *ku* 'whither?', *kutar* 'how?'; Kamassian *kojət* 'what kind of?', *kammõn* 'when?', *kõdaʔ* 'how?'. Yukaghir (Southern / Kolyma) *hadi* 'which?', *hodiet* 'why?', *hon* 'where?, whither?', *hot* 'from where?, whence?'.

D. Proto-Altaic **kʰa(y)* interrogative pronoun: 'who?, what?': Proto-Tungus **χia* (**χai*) 'who?, what?' > Manchu *ai*, *ya* 'who?, what?, which?'; Evenki *ḗ* 'who?', *ḗkūn* 'what?'; Lamut / Even *ā̄q* 'what?'; Negidal *ḗχun*, *ḗkun* 'who?, what?', *ḗwa* 'what?'; Ulch *χay* 'what?'; Orok *χai* 'what?'; Nanay / Gold *χaị* 'what?'; Solon *ī* 'what?'. Proto-Mongolian **ken*, **ka-* 'who?, which?' > Written Mongolian *ken* 'who?, which?'; Khalkha *χen* 'who?, which?'; Buriat *χen* 'who?, which?'; Kalmyk *ken* 'who?, which?'; Ordos *ken* 'who?, which?'; Moghol *ken* 'who?, which?'; Dagur *ken*, *χen* 'who?, which?', *χā-*, *hā-* 'where?'; Monguor *ken* 'who?, which?'. Proto-Turkic **kem-*, **ka-* 'who?, which?' > Old Turkic (Old Uighur) *kem* 'who?', *qayu*, *qanu* 'which?'; Karakhanide Turkic *kem*, *kim* 'who?', *qayu* 'which?'; Turkish *kim* 'who?'; Gagauz *kim* 'who?'; Azerbaijani *kim* 'who?'; Turkmenian *kim* 'who?', *qay* 'which?'; Uzbek *kim* 'who?', *qay* 'which?'; Uighur *kim* (dial. *kem*) 'who?', *qay* 'which?'; Karaim *kïm* 'who?'; Tatar *kem* 'who?', *qay* 'which?'; Bashkir *kem* 'who?', (dial.) *qay* 'which?'; Kirghiz *kim* 'who?', *qay* 'which?'; Kazakh *kim* 'who?', *qay* 'which?'; Noghay *kim* 'who?'; Oyrot (Mountain Altai) *kem* 'who?', *qay* 'which?'; Tuva *qïm* 'who?', *qayï* 'which?'; Chuvash *kam* 'who?'; Yakut *kim* 'who?', *χaya* 'which?'; Dolgan *kim* 'who?', *kaya* 'which?'.

E. Proto-Eskimo **ki(na)* 'who': Alutiiq Alaskan Yupik *kinaq* 'who'; Central Alaskan Yupik *kina* 'who'; Naukan Siberian Yupik *kina* 'who'; Central Siberian Yupik *kina* 'who'; Sirenik *kin* 'who'; Seward Peninsula Inuit *kina* 'who'; North Alaskan Inuit *kin*^{y}*a* 'who'; Western Canadian Inuit *kina* 'who'; Eastern Canadian Inuit *kina* 'who'; Greenlandic Inuit *kina* 'who'. Aleut *kiin* 'who'. Proto-Eskimo **kitu* 'who' or 'which': Alutiiq Alaskan Yupik *kitu-* 'who'; Central Alaskan Yupik *kitu-* 'who'; Naukan Siberian Yupik *kitu-* 'who'; Central Siberian Yupik *kitu-* 'who'; Seward Peninsula Inuit *kitu* 'which'; North Alaskan Inuit *kisu* 'which'; Eastern Canadian Inuit *kituuna* 'who is that'; Greenlandic Inuit (North Greenlandic / Polar Eskimo) *kihu* 'what'. Proto-Inuit **qanuq* 'how' > Seward Peninsula Inuit *qanuq* 'how'; North Alaskan Inuit *qanuq* 'how'; Western Canadian Inuit *qanuq* 'how'; Eastern Canadian Inuit *qanuq* 'how'; Greenlandic Inuit *qanuq* 'how'. Proto-Eskimo **qaŋa* 'when (in past)': Sirenik *qaŋən* 'when (in past?)'; Seward Peninsula Inuit *qaŋa* 'when (in past)'; North Alaskan Inuit *qaŋa* 'when (in past)'; Western Canadian Inuit *qaŋa* 'when (in past)'; Eastern Canadian Inuit *qaŋa* 'when'; Greenlandic Inuit *qaŋa* 'when (in past)'. Aleut *qana-* 'which, where', *qanayaam* 'when', *qanaaŋ* 'how many'. Proto-Eskimo **qaku* 'when (in future)': Alutiiq Alaskan Yupik *qaku* 'when (in future)'; Central Alaskan Yupik *qaku* 'when (in future)'; Naukan Siberian Yupik *qaku* 'when'; Central Siberian Yupik *qakun* 'when (in future)'; Sirenik *qaku* 'when'; Seward Peninsula Yupik *qaɣu(n)*, *qaɣuʀun* 'when (in future)'; North Alaskan Inuit *qakuɣu* 'when (in future)'; Western Canadian Inuit (Siglit) *qaku(ɣu)* 'when (in future)'; Eastern Canadian Inuit *qaku* 'when (at last, after lengthy waiting)'; Greenlandic Inuit *qaquɣu* 'when (in future)'. Proto-Yupik-Sirenik **qayu(q)* 'how' > Alutiiq Alaskan Yupik *qayu* 'how'; Central Alaskan Yupik *qayumi* 'indeed, as expected'; Naukan Siberian Yupik *qay* 'I wonder, is that so?', *qaywa* 'really?, is that so?'; Central Siberian Yupik *qayuq* 'how'; Sirenik *qayŋun* 'really?'.

Again, there is no evidence for reconstructing an initial glottalic in the Nostratic proto-form.

Note that, in the above two examples, the etymologies remain valid, it is just the Proto-Nostratic reconstructions proposed by the Moscow School that are wrong. Of course, given the revised sound correspondences, new etymologies suggest themselves that were not apparent to the Moscovites, while some of the etymologies based upon the incorrect sound correspondences must now be discarded. This notwithstanding, the vast majority of work produced by Illič-Svityč some forty years ago holds up quite well.

In closing, we may note that Alexis Manaster Ramer (1997:94—96) arrived at the same conclusions reached here regarding the need to reexamine the Nostratic sound correspondences proposed by Illič-Svityč (and, by implication, Dolgopolsky as well) in light of typological considerations. Specifically, he writes:

> 6.1. Finally, quite recently, I decided to see what would happen if one counted up the occurrences of the different stops (voiceless vs. voiced vs. glottalized as well as labial vs. coronal vs. velar) reconstructed for Nostratic by Illich-Svitych. I only performed the experiment on root-initial stops, with the following results: (they are given as approximations because there is a problem arriving at exact figures given that there [are] some cases where it is difficult to tell whether one is dealing with a single Nostratic form or two, or whether a particular form should begin with this or that stop):
>
> | | | |
> |---|---|---|
> | *b 50+ | *d 20+ | *g 40+ |
> | *p 15+ | *t 15+ | *k 50+ |
> | *p' 40+ | *t' 30+ | *k' 60+ |
>
> The first observation (see Manaster Ramer in press a) was that ... the relative frequencies of the three phonation types (voiced, voiceless, glottalized) posited for Proto-Nostratic stops, as reflected in the sets of cognates compiled by Illich-Svitych, seem to be inconsistent with typological predictions. Specifically, at least in initial position, the series of stops reconstructed as glottalized is much more frequent at all points of articulation than the series reconstructed as (plain) voiceless.
>
> Since one expects glottalized stops to be more marked and hence less frequent than plain voiceless, in particular, something was amiss. However, just as in the case of the clusters and affricates discussed above, the solution turned out to be quite simple. Given the markedness considerations, I would suggest that the "glottalized" series was actually plain voiceless in Proto-Nostratic, while the "voiceless" series represented some more marked phonation type, glottalized or perhaps aspirated. This is consistent with the fact that the Nostratic series Illich-Svitych wrote as "glottalized" is in fact realized as glottalized only in parts of Afro-Asiatic and in Kartvelian, and in the latter it is easy to imagine that this could be a contact-induced development.
>
> This reinterpretation of Nostratic ... naturally calls to mind the glottalic theory of Indo-European. As it happens, the stop series reconstructed by Illich-Svitych as plain voiceless and by me as glottalized (or aspirated) comes out in Proto-Indo-European as that series of stops which is traditionally reconstructed as voiced (media) but which many scholars have recently interpreted as glottalized.
>
> | Nostratic (Illich-Svitych) | Nostratic (Manaster Ramer) | Indo-European (Traditional) | Indo-European (Glottalic) |
> |---|---|---|---|
> | *t | *t' (or *t^h) | *d | *t' |
> | *t' | *t | *t | *t |
> | *d | *d | *dh | *d |
>
> Totally unexpectedly, typological considerations provide us with arguments for reinterpreting the Nostratic stop series in a way that fits quite well with the glottalic theory of Indo-European. Of course, there is no reason in general to expect the phonetics of related languages and proto-languages to agree in this way, and such a convergence cannot be regarded as a criterion or an argument for relatedness among languages, since that would entail the "misuse of similarity" which Hamp (1992) cautions against. But it is not an unwelcome development when it occurs.

Appendix

This Appendix contains a sampling of the evidence from the Nostratic daughter languages that supports the sound correspondences for glottalics I have proposed (only for stops and only in initial position). Here, I will just give the reconstructed proto-forms for each daughter language (except for Dravidian and Etruscan) — the full body of supporting data, along with references to the relevant literature, can be found in my forthcoming book *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology, and Vocabulary.* Much of this supporting material is also listed in my 1994 co-authored book *The Nostratic Macrofamily: A Study in Distant Linguistic Relationship.*

Proto-Nostratic *p':

1. Proto-Nostratic **p'ap'a-* 'old man, old woman':

   A. Proto-Kartvelian **p'ap'-* 'grandfather';
   B. Proto-Indo-European (f.) **p'ā̆p'aA* > **p'ā̆p'ā* 'old woman'.

2. Proto-Nostratic **p'ul-* (~ **p'ol-*) '(vb.) to swell; (n.) swelling, hump, lump, bulge; (adj.) swollen, round, bulbous':

   A. Proto-Afrasian **p'ul-* 'to swell', (reduplicated) **p'ul-p'ul-* '(vb.) to swell; (adj.) swollen, round';
   B. Proto-Indo-European **p'ul-*, **p'ol-* 'swollen, round', (reduplicated) **p'ulp'ul-*, **p'olp'ol-* (dissimilated to **p'ump'ul-*, **p'omp'ol-*; **p'omp'ul-*);
   C. Proto-Altaic **pula-* (~ *-o-*) 'to swell'.

3. Proto-Nostratic **p'ut'-* (~ **p'ot'-*) '(vb.) to cut, tear, break, or pull off or apart; (n.) cut-off, pulled-off, torn-off, or broken-off piece or part':

   A. Afrasian: Proto-Semitic **bat'-* 'to cut, tear, break, or pull off or apart' (with numerous extensions);
   B. Dravidian: Kolami *put-* (*putt-*) 'to cut in pieces, to pluck (flower), to break (rope)'; Naikṛi *put-* 'to cut, to pluck'; Naiki (of Chanda) *put-* 'to be cut, to break (intr.)', *putuk-* 'to cut to pieces'; Kuṛux *pud*^{u}*gnā* (*pudgas*) 'to pluck out (hair, etc.), to strip (fowl) by plucking';
   C. Proto-Kartvelian **p'ut'wn̥-* 'to pluck (poultry)'.

Proto-Nostratic *t':

1. Proto-Nostratic **t'ab-* (~ **t'əb-*) 'to be or become warm; to make warm, to heat up':

   A. Proto-Afrasian **t'ab-* 'to be or become warm; to make warm, to heat up';
   B. Proto-Kartvelian **t'eb-/*t'b-* 'to warm, to heat; to warm oneself'.

Note: The variant Proto-Nostratic _*t^{h}ep^h-_ 'to warm, to burn' is not related to the above.

2. Proto-Nostratic _*t'aħ-_ (~ _*t'əħ-_) '(vb.) to break, to split; to crush, to grind, to pound; (n.) break, split, division; anything ground or pulverized':

   A. Proto-Afrasian _*t'aħ-_ 'to break, to split; to crush, to grind, to pound';
   B. Proto-Kartvelian _*t'ex-_ 'to break';
   C. Proto-Indo-European _*t'eħ͜h̲-_ [_*t'aħ͜h̲-_] > _*t'ā-_ 'to cleave, to split, to divide'; (extended form) _*t'eħ͜h̲-y/i-_ [_*t'aħ͜h̲-y/i-_].

3. Proto-Nostratic _*t'al-_ (~ _*t'əl-_) '(vb.) to drip, to fall in drops, to sprinkle, to wet, to moisten; (n.) dew, (rain) drop, drizzle':

   A. Proto-Afrasian _*t'al-_ ~ _*t'ul-_ (vb.) 'to drip, to fall in drops, to sprinkle, to wet, to moisten', (n.) _*t'al-_ 'dew, drop';
   B. Proto-Indo-European _*t'el-/*t'ol-_ 'to drip, to fall in drops, to sprinkle, to wet, to moisten'.

4. Proto-Nostratic _*t'al-_ (~ _*t'əl-_) '(vb.) to stretch out, to extend; (n.) length; height; (adj.) long, tall; high':

   A. Afrasian: Proto-Semitic _*t'a/wa/l-_ 'to stretch out, to extend'; Proto-Semitic (reduplicated) _*t'al-t'al-_ 'to throw'; Proto-Semitic _*na-t'al-_ 'to lift';
   B. Proto-Indo-European (_*t'el-/*t'ol-/*t'l̥-_ 'to stretch, to extend, to lengthen':) (extended forms) _*t'l̥-H-g^{h}o-_ 'long', _*t'l-e-Eg^h-_ > _*t'lēg^h-_ '(vb.) to stretch, to extend, to lengthen; (n.) length'.

5. Proto-Nostratic _*t'al-_ (~ _*t'əl-_) 'to lick':

   A. Proto-Kartvelian _*t'lek'-/*t'lik'-_ 'to lick, to lick oneself';
   B. Proto-Altaic _*tălV-_ 'to lick'.

6. Proto-Nostratic _*t'an-_ (~ _*t'ən-_) '(vb.) to fill, to stuff, to pack tightly together; (adj.) closely packed or pressed together; close, thick, dense':

   A. Afrasian: Egyptian _dns_ 'to be loaded heavily', _dns_ 'weight, load, burden; heavy', _dnsw_ 'weights';
   B. Proto-Kartvelian _*t'en-_ 'to fill, to stuff, to pack (tight) with', (?) (reduplication of the simple verbal stem _*t'en-_) _*t'it'in-_ 'to stuff, to fill tight';
   C. Proto-Indo-European _*t'n̥s-u-_ 'closely packed or pressed together; thick, dense'.

7. Proto-Nostratic _*t'ap^h-_ (~ _*t'əp^h-_) 'to strike, to beat, to pound':

   A. Proto-Afrasian _*t'ap-_ 'to strike, to hit';

B. Proto-Indo-European _*t’ep_^{h}_-/*t’op_^{h}_-_ ‘to pound, to trample’;
C. Proto-Altaic _*tāp_^{h}_V-_ ‘to strike, to press’.

8. Proto-Nostratic _*t’aq’-_ (~ _*t’əq’-_) ‘(vb.) to cover, to protect; (n.) covering’:

A. Proto-Kartvelian _*t’q’aw-_ ‘skin, hide’;
B. Proto-Indo-European _*(s)t’ek’-/*(s)t’ok’-_ > (with regressive deglottalization [see above]) _*(s)t_^{h}_ek’-/*(s)t_^{h}_ok’-_ ‘to cover’.

9. Proto-Nostratic _*t’ar-_ (~ _*t’ər-_) ‘(vb.) to tear, to rend, to cut, to sever; (n.) rip, tear, cut, slice’:

A. Proto-Afrasian _*t’Vr-_ ‘to take away’ > Proto-Semitic _*ʔa-t’ar-_ ‘to take away’ (_*ʔa-_ is a prefix) — for the semantics, cf. Gothic _dis-tairan_ ‘to tear down, to remove’ and _ga-tarnjan_ ‘to rob, to take away’, which are derived from Proto-Indo-European _*t’er-/*t’or-/*t’r̥-_ ‘to tear, to rend, to flay’ cited below;
B. Dravidian: Tamil _taṟi_ (_-pp-_, _-tt-_) ‘to lop, to chop off, to cut off’, _taṟi_ (_-v-_, _-nt-_) ‘to be cut off, broken’, _taṟi_ ‘a cutting off, wooden post, stake, weaver’s loom, a kind of axe’, _taṟikai_ ‘a kind of axe, chisel’; Malayalam _taṟikka_ ‘to cut down’, _taṟi_ ‘pot, hedge-stake, stick, cutting, weaver’s loom’; Kota _tayr-_ (_tarc-_) ‘to cut (using an implement with one hand); to cut a path through jungle’; Kannaḍa _taṟi_, _taṟe_ ‘to strip off, to cut off, to cut’, _taṟi_ ‘cutting, slaughter; stake, post, sharp knife or sword’; Koḍagu _tari-_ (_tarip-_, _taric-_) ‘to chop to small bits’, _tarip_ ‘cutting’; Tuḷu _taripuni_ ‘to lop off, to clear (jungle)’; Telugu _taṟugu_, _targu_, _taṟuvu_, _tarvu_ ‘to slice, to chop’; Kolami _targ-_ (_tarakt-_) ‘to cut, to cut off’; Naikṛi _targ-_ ‘to cut’; Kuṛux _tārnā_ (_tāryas_) ‘to fell (tree), to lop off (bough)’; Malto _táre_ ‘to cut down, to fell’, _tare_ ‘to break (as a stick), to injure’;
C. Proto-Indo-European _*t’er-/*t’or-/*t’r̥-_ ‘to tear, to rend, to flay’.

10. Proto-Nostratic _*t’aw-_ (~ _*t’əw-_) ‘(vb.) to go, to leave, to go away; to let go; (adj.) far away, remote, at a distance’:

A. Proto-Afrasian _*t’aw-_ ‘to go, to go away’;
B. Proto-Kartvelian _*t’ew-_ ‘to leave, to let go’;
C. Proto-Indo-European _*t’ew(A)-/*t’ow(A)-/*t’u(A)-_ ‘to go, to leave, to go away; far off, far away, distant’.

11. Proto-Nostratic _*t’aw-_ (~ _*t’əw-_) ‘(vb.) to hit, to strike; (n.) stroke, blow, injury, harm, damage’:

A. Proto-Afrasian _*t’aw-_ ‘to hit, to strike’;
B. Proto-Indo-European _*t’ew-/*t’ow-/*t’u-_ ‘to hit, to strike’.

Note: Also found in Northwest Caucasian: cf. Proto-Circassian _*t’awə_ ‘to bump one’s head’ > Temirgoy _ya-t’awə_ ‘to bump one’s head’.

12. Proto-Nostratic (Eurasiatic only) **t'ay-* '(elder) male in-law, (elder) male relative':

A. Proto-Indo-European **t'ay-wer-*/**t'ay-wr̥-* 'brother-in-law on husband's side';
B. Proto-Altaic **tāyV* 'elder male in-law, elder male relative'.

13. Proto-Nostratic **t'ay-* (~ **t'əy-*) or **t'iy-* (~ **t'ey-*) '(vb.) to shine, to gleam, to be bright, to glitter, to glow; to burn brightly; (n.) light, brightness, heat':

A. Dravidian: Tamil *tī*, *tīy* 'to be burnt, charred, blighted'; Malayalam *tī* 'fire'; Kota *ti·y-* (*ti·c-*) 'to be singed, roasted'; Toda *ti·y-* (*ti·s-*) 'to be singed', *ti·y-* (*ti·c-*) 'to singe, to roast'; Kannaḍa *tī* 'to burn, to scorch, to singe, to parch'; Telugu *tīṇḍrincu*, *tī̃ḍirincu* 'to shine', *tīṇḍra* 'light, brightness, heat'; Brahui *tīn* 'scorching, scorching heat', *tīrūnk* 'spark';
B. Proto-Indo-European **t'ey-*/**t'oy-*/**t'i-* 'to shine, to be bright';
C. Etruscan *tin* 'day', *tiu*, *tiv-*, *tiur* 'moon, month'; Rhaetic *tiu-ti* 'to the moon'.

14. Proto-Nostratic **t'eʔ-* 'to say, to speak':

A. Proto-Indo-European **t'eʔ-* (> **t'ē-*) 'to say, to speak';
B. Proto-Altaic **tē-* 'to say, to sound'.

15. Proto-Nostratic **t'il-* (~ **t'el-*) '(vb.) to say, to tell; to recount, to list, to enumerate; (n.) talk, speech, discourse, tale':

A. Proto-Indo-European **t'el-* (secondary *o*-grade form: **t'ol-*) '(vb.) to say, to tell, to recount; to list, to enumerate; (n.) talk, speech, language; list, enumeration';
B. Proto-Eskimo **təli-* 'to tell someone to do something'.

16. Proto-Nostratic **t'il-* (~ **t'el-*) 'tongue, language' (derivative of **t'il-* 'to say, to tell; to recount, to list, to enumerate' [see directly above]):

A. Dravidian: Kui *tlēpka* (< *tlēk-p-*, *tlēkt-*) 'to put out the tongue, to thrust forth from a cavity'; Kuwi *tekh-* in: *vendōri tekhmū* 'put out your tongue!';
B. Proto-Indo-European (**t'l̥gʰuA-*/**t'l̥gʰweA-* >) **t'n̥gʰū-*/**t'n̥gʰwā-* 'tongue' (with widely different reflexes in the daughter languages due to taboo);
C. Proto-Altaic **tilV* 'tongue, voice';
D. Proto-Chukchi-Kamchatkan (reduplicated) **jilə(jil)* (if from **tilə(til)*) 'tongue'.

17. Proto-Nostratic **t'oH-* '(vb.) to give, to bring; (n.) giving, gift, present':

A. Proto-Indo-European (**t'oH-C-* >) **t'ō-* 'to give'; (extended form) **t'oH-w-* (> **t'ō̆w/u-*);
B. Proto-Uralic **toxe-* 'to give, to bring'.

18. Proto-Nostratic **t'uk'-* (~ **t'ok'-*) '(vb.) to knock, to beat, to strike, to pound, to trample; (n.) knock, thump, blow, stroke':

    A. Proto-Afrasian **t'uk'-*, **t'ok'-* 'to knock, to beat, to strike, to pound';
    B. Dravidian: Tamil *tukai* 'to tread down, to trample on, to bruise or destroy by treading, to pound in a mortar, to mash, to vex'; Kannaḍa *tōku* 'to beat, to strike'; Tuḷu *tōku* 'collision'; Manḍa *tug-* (*tukt-*) 'to trample'; Pengo *tog-* (*tokt-*) 'to tread on, to step on'; Kui *tōga* (*tōgi-*) 'to kick'; Kuṛux *tōknā* 'to stamp violently with one foot or with both feet (as in *jatra* dance)';
    C. Proto-Kartvelian **t'k'ač-* 'to hit, to strike', **t'k'eč-/*t'k'ič-* 'to beat, to hit, to strike', **t'k'eb-/*t'k'b-* 'press, to squeeze', **t'k'ep'-* 'to press, to trample';
    D. Proto-Indo-European **t'ok'-* > (with regressive deglottalization) **tʰok'-* (secondary *e*-grade form: **tʰek'-*) 'to knock, to beat, to strike';
    E. Proto-Finno-Permian **tuk3-* (**tuγ3-*) 'to break, to crush';
    F. Altaic: Mongolian *tuyila-* 'to strike with the feet, to rear, to buck (of a horse)'; Khalkha *tuil-* 'to strike with the feet, to rear, to buck (of a horse)'. Turkic: Sagai (dialect of Khakas) *tuγula-* 'to strike with the feet, to rear, to buck (of a horse)'.

19. Proto-Nostratic (Eurasiatic only) **t'ulʸ-* 'wedge, peg':

    A. Proto-Indo-European **t'ul-* 'pin, wedge, peg';
    B. Proto-Finno-Volgaic **tola* 'wedge, peg';
    C. Proto-Altaic **ti̯ūlʸu* 'wedge, peg'.

20. Proto-Nostratic **t'uq'ʷ-* (~ **t'oq'ʷ-*) '(vb.) to be dark, cloudy, dusty, dirty, sooty, smoky; (n.) darkness, (dark) cloud, dust, dirt, soot, smoke':

    A. Proto-Afrasian **t'o(o)k'ʷ-* '(vb.) to be dark, cloudy, dusty, sooty, smoky; (n.) fog, cloud, darkness, soot, smoke';
    B. Dravidian: Tamil *tukaḷ* 'dust, particle of dust, pollen; fault, moral defect'; Telugu *dūgaṟa* 'dust, dirt, soot'; Kolami *tu·k* 'dust, earth, clay'; Naikṛi *tūk* 'earth, clay'; Parji *tūk*, *tūkuḍ* 'earth, clay, soil'; Gadba (Ollari) *tūkuḍ* 'earth, clay';
    C. Proto-Chukchi-Kamchatkan **təqi-* '(vb.) to smoke; (n.) smoke'.

Proto-Nostratic *k':

1. Proto-Nostratic **k'ab-* (~ **k'əb-*) '(vb.) to seize, to take hold of; to seize with the teeth, to bite; (n.) seizure, grasp, grip, hold; bite':

    A. Proto-Afrasian **k'ab-* 'to seize, to take hold of';
    B. Dravidian: Tamil *kappu* (*kappi-*) 'to gorge, to cram into the mouth', *kavvu* (*kavvi-*), *kauvu* (*kauvi-*) '(vb.) to seize with the mouth, to grasp with eagerness; (n.) bite, seizing by the mouth (as dog), eating'; Malayalam *kauvuka* 'to seize with the mouth, to bite', *kappuka*, *kammuka* 'to snap at, to eat as a dog or a madman'; Koḍagu *kabb-* (*kabbi-*) 'to seize with wide-open mouth (of dogs, tigers, etc.)';

Tuḷu *kappuni* 'to eat greedily'; Telugu *kavvu* 'to seize by the mouth'; Pengo *kap-* 'to bite'; Manḍa *kap-* 'to bite'; Kui *kappa* (*kapt-*) 'to swallow liquid hastily, to gulp, to drink'; Kuṛux *xappnā* 'to swallow, to drink', *habkaʔānā* 'to bite', *habkā* 'a bite';

C. Proto-Kartvelian **k'b-en-*, **k'b-in-* 'to bite', **k'b-il-* 'tooth'.

2. Proto-Nostratic **k'ad-* (~ **k'əd-*) '(vb.) to tie, to fasten; to build, to construct; (n.) tie, band, fastening':

A. Afrasian: Egyptian *qd* 'to build, to fashion (pots)', *qd* 'to use the potter's wheel', *qd* 'builder, potter', *ỉqdw* 'potter, mason, creator'; Coptic *kōt* [**ⲕⲱⲧ**] 'to build, to form', *ekōt* [**ⲉⲕⲱⲧ**] 'builder, mason, potter', *se-kōt* [**ⲥⲉ-ⲕⲱⲧ**] 'potter's workshop';
B. Dravidian: Tamil *kaṭṭu* (*kaṭṭi-*) '(vb.) to tie, to fasten, to build, to wear, to put on, to bind by spells, to marry, to shut up, to store, to hug, to compare with, to be equal; (n.) tie, band, fastening, regulations, custom, building, marriage, bundle, packet, dam, causeway', *kaṭṭaṭam* 'building, binding of a book, setting of a jewel', *kaṭṭaṇam* 'building', *kaṭṭaḷai* 'code, rule, regulations', *kaṭṭai* 'dam'; etc.;
C. Proto-Kartvelian **k'ed-/*k'd-* 'to build, to construct', **k'ed-el-* 'wall';
D. Proto-Altaic **kadu* 'a kind of harness (bridle)'.

3. Proto-Nostratic **k'ak'-* (onomatopoeic) '(vb.) to cackle, to chatter; (n.) crackling sound':

A. Proto-Afrasian **k'ak'-* 'to cackle, to make a noise';
B. Dravidian: Kui *kapka* (< **kak-p-*, *kakt-*) 'to laugh, to laugh at, to ridicule'; Kuwi *kak-* 'to laugh', *kakpinai* 'to joke';
C. Proto-Kartvelian **k'ak'a-n-* 'to cackle';
D. Proto-Indo-European **k'ak'-* 'to cackle, to chatter'.

Note: Also found in Northwest Caucasian: cf. Proto-Circassian **k'ak'a* 'to chirp' > Kabardian *k'āk'a* 'to chirp'.

4. Proto-Nostratic **k'ak'-* (onomatopoeic bird name) 'partridge' (derivative of **k'ak'-* 'to cackle, to chatter'):

A. Proto-Afrasian **k'ak'-* 'partridge';
B. Dravidian: Kolami *kakkare* 'partridge'; Parji *kākral* 'partridge'; Gondi *kakrā̆nj* 'partridge';
C. Proto-Kartvelian **k'ak'ab-* 'partridge';
D. Altaic: Proto-Turkic **käkälik* 'partridge' > Turkish *keklik* 'red-legged partridge';
E. Proto-Chukchi-Kamchatkan **kakac(o)* 'a kind of bird'.

Note: Loanwords are found in Indo-European: Hittite *kakkapa-* onomatopoeic bird name; Greek κακκάβη 'partridge' (cf. Akkadian *kakkabānu* name of a bird).

5. Proto-Nostratic *_k’al-_ ‘(vb.) to feed, to nourish; (n.) nourishment, sustenance, nutriment’:

   A. Afrasian: Proto-Semitic *_k’al-ab-_ ‘to feed, to nourish’;
   B. Proto-Indo-European *_k’al-_ ‘to (breast-)feed, to nourish, to satisfy’, *_k’(a)lak_^{h}_t_^{h}_-_ ‘nourishment, milk’.

6. Proto-Nostratic *_k’al-_ ‘stone, rock’:

   A. Dravidian: Tamil _kal_ (_kar̲-_, _kan̲-_) ‘stone, pebble, boulder, precious stone, milestone’; Malayalam _kal_, _kallu_ ‘stone, rock, precious stone’, _kalla_ ‘glass beads’, _kallan_ ‘mason; hard-hearted’; Kolami _kal_ ‘stone, milestone’; Toda _kal_ ‘milestone, bead’, _kalïr_ ‘round river stone’; Kannaḍa _kal_, _kalu_, _kallu_ ‘stone; hard, stiff state of mind’; Koḍagu _kallï_ ‘stone’; Tuḷu _kallu̥_ ‘stone’; Telugu _kallu_ ‘stone’; Naikṛi _khalbada_ ‘stone slab for pounding’; Parji _kel_ ‘stone’; Gondi _kal_, _kall(i)_, _kalu_ ‘stone’; Koṇḍa _kalu_ ‘stone’; Pengo _kal_ ‘stone’; Brahui _xal_ ‘stone, boulder’;
   B. Proto-Kartvelian *_k’l̥de-_ ‘rock, cliff’;
   C. Proto-Indo-European *_k’(e)l-_ ‘rock, stone’;
   D. (?) Uralic: Finnish _kallio_ ‘rock’, _rantakallio_ ‘cliff’; Estonian _kalju_ ‘rock, boulder’, _rannakalju_ ‘cliff, crag’, _kaljune_ ‘rocky’; Lapp / Saami _kallo_ ‘rock’. These forms are usually considered to be loans from Germanic (cf. Gothic _hallus_ ‘rock’; Old Icelandic _hallr_ ‘big stone’, _hella_ ‘flat stone, slab of rock’; Old English _heall_ ‘rock’);
   E. Proto-Eskimo *_qaluʀ_ ‘rock’.

7. Proto-Nostratic *_k’al-_ (~ *_k’əl-_) ‘(vb.) to take away, to remove, to deprive of; to decrease, to diminish, to reduce; to be or become reduced or diminished; (adj.) little, scanty, sparse, meager, insufficient, lacking, short of, wanting, needy’:

   A. Proto-Afrasian *_k’al-_ ‘to take away, to remove, to deprive of; to decrease, to diminish, to reduce; to be or become reduced or diminished’;
   B. Proto-Kartvelian *_k’el-_/*_k’l-_ ‘to lack, to be short of’, (Georgian-Zan) *_m-k’l-e-_ ‘missing, deprived’;
   C. Proto-Finno-Ugrian *_kelke-_ ‘to be necessary; must, ought to’.

8. Proto-Nostratic *_k’al-_ (~ *_k’əl-_) ‘(vb.) to burn, to warm, to cook, to roast; (n.) cooking, roasting, baking; glowing embers’:

   A. Proto-Afrasian *_k’[a]l-_ ‘to burn, to roast’;
   B. (?) Dravidian: Malayalam _kāḷuka_ ‘to burn, to flame’, _kāḷal_ ‘high flame, love-fever’; Telugu _kālu_ ‘to burn; to be burnt, scalded, scorched, baked’, _kālupu_ ‘burning, setting on fire, roasting, baking’, _kālcu_ ‘to burn (tr.), to set fire to, to scald, to singe, to scorch, to char, to bake’; Parji _kāl-_ ‘to smart’;
   C. Proto-Indo-European *_k’el(H)-_/*_k’ol(H)-_/*_k’l̥(H)-_ ‘to burn, to scorch, to char’ > Common Germanic *_kolan_ or *_kulan_ ‘coal, charcoal’.

9. Proto-Nostratic **k'al-* (~ **k'əl-*) (vb.) 'to move, to tremble, to shake, to agitate, to stir, to mix; (n.) agitation, trembling, perturbation, distress, confusion, uneasiness, disturbance':

    A. Proto-Afrasian **k'[a]l-* 'to move, to tremble, to shake, to agitate, to stir, to mix';
    B. Dravidian: Tamil *kalaṅku* (*kalaṅki-*) 'to be stirred up, agitated, ruffled (as water), confused, abashed', *kalakku* (*kalakki-*) 'to confuse', *kalakkam*, *kalakku* 'being agitated (as surface of water), discomposure, distress, perplexity', *kalāvu* (*kalāvi-*) 'to be perturbed, confused, displeased, angry', *kalaṅkal* 'turbidity, muddiness, muddy water, perturbation', *kali* 'perturbation, discomposure, uneasiness, war, dissension, strife'; etc.

10. Proto-Nostratic **k'al-* '(vb.) to come into being, to be born; (n.) existence, presence, appearance, birth':

    A. Proto-Afrasian **k'al-* 'to give birth, to beget';
    B. Dravidian: Tamil *kala* 'to appear, to come into being, to spread (as news)', *kali* '(vb.) to grow luxuriantly, to sprout, to come into being, to appear, to increase; (n.) flourishing, prospering'; Telugu *kalugu* 'to accrue, to happen, to occur, to be produced or caused, to be born, to be, to exist, to be able', *kaligincu* 'to cause, to produce, to effect, to bring about', *kala* 'existing, true, actual, possessing, having', *kalimi* 'existence, presence; possessions, wealth'; Kolami (neg.) *kal-*, *kalt-* (present-future paradigm, present-future or past in meaning) 'possibly be, may be', *kall-*, *kal-* 'to do'; Konḍa *kalgi-* 'to accrue (as prosperity), to happen'; Kuwi *kalg-* 'to get, to become, to accrue'. (?) Pengo *kaṛde* 'boy, son' (< **kaḷde* ?); Manḍa *kaṛde* 'boy';
    C. Proto-Indo-European **k'al-* 'pregnant, young of animals';
    D. Etruscan *clan* (pl. *clenar*) 'son', *clante*, *clanti*, *clanθi* 'adoptive (?) son'; Rhaetic *kalun* 'son'. Semantic development as in Burji *k'al-a* 'son, male child, young of animals' from Proto-Afrasian **k'al-* 'to give birth, to beget' cited above.

11. Proto-Nostratic **k'an-* (~ **k'ən-*) '(vb.) to get, to acquire, to create, to produce, to beget; (adj.) born, begotten, produced; (n.) birth, offspring, child, produce':

    A. Proto-Afrasian **k'an-* 'to get, to acquire, to possess, to create, to produce';
    B. Dravidian: Tamil *kan̲r̲u* 'calf, colt, young of various animals, sapling, young tree'; Malayalam *kannu* 'young of cattle (esp. buffalo calf), young plantain trees around the mother plant'; Kannaḍa *kanda* 'young child', *kandu* 'calf, young plantain trees around the mother plant'; Telugu *kandu* 'infant', *kanduvu* 'child', *kanu* 'to bear or bring forth, to beget', *kanubadi* 'produce', *kāncu* 'to bear, to produce, to bring forth', *kānupu* 'bringing forth a child'; Konḍa *kās-* 'to bring forth young (of human beings), to bear children'; Kuṛux *xadd* 'child, young animal or plant'; Malto *qade* 'son'; Brahui *xaning* 'to give birth to';

C. Proto-Indo-European **k'en-*/**k'on-*/**k'n̥-* 'to beget, to produce, to create, to bring forth'.

12. Proto-Nostratic **k'an-* (~ **k'ən-*) 'jaw, cheek':

A. Dravidian: Tamil *kaṉṉam* 'cheek, ear'; Malayalam *kannam* 'cheek, jaw'; Kannaḍa *kanna* 'the upper cheek';
B. Proto-Indo-European **k'en-u-* 'jaw, cheek'.

13. Proto-Nostratic **k'aŋ-* (~ **k'əŋ-*) '(vb.) to bend, twist, turn, or tie together; (n.) wreath, rope, cord, fiber, tie, band, string':

A. Proto-Afrasian **k'an-* 'to bend, twist, turn, or tie together';
B. Dravidian: Tamil *kaṇṇi* 'wreath, garland, neck-rope for bullock, rope', *kaṇṇu* (*kaṇṇi-*) 'to be attached to, to be fastened to'; Kota *kayṇ* 'yoke-rope for bullock'; Kannaḍa *kaṇṇi* 'rope, cord, neck-rope'; (?) Tuḷu *kaṇṇi* 'fiber'; Telugu *kanne-tāḍu* 'neck-rope (of calves, oxen)'; Konḍa *kane* 'a rope used to fasten cattle. Tamil *kaṇṇi* 'snare, noose, net, knot, tie'; Malayalam *kaṇi* 'snare, gin', *kaṇikka* 'to lay a snare', *kaṇṇi* 'link of a chain, mesh of a net', *keṇi* 'snare, trap, stratagem', *keṇikka* 'to entrap'; Kannaḍa *kaṇi* 'knot, tie', *kaṇaya*, *kaṇe* 'the knot which fastens a garment around the loins', *keṇi* 'trick'; Koḍagu *këṇi* 'bird-trap (bent sapling and noose with bait); trickiness, cunning', *këṇi* (*këṇiv-*, *këṇiñj-*) 'to get stuck, caught'; (*këṇip-*, *këṇic-*) 'to entangle, to get into trouble'; Tuḷu *keṇi* 'stratagem', *kiṇi* 'wit, cunning';
C. Proto-Indo-European (**k'en-*/**k'on-*)**k'n-* 'to bend, twist, turn, or tie together';
D. Proto-Chukotian **kæŋ(æt)-* 'to bend'.

14. Proto-Nostratic **k'aŋ-* (~ **k'əŋ-*) 'knot, knob, joint' (derivative of **k'aŋ-* 'to bend, twist, turn, or tie together'):

A. Dravidian: Tamil *kaṇ* 'joint in bamboo or cane', *kaṇu* 'joint of bamboo, cane, etc., knuckle, joint of the spine, vertebra', *kaṇukkai* 'wrist', *kaṇukkāl* 'ankle'; Malayalam *kaṇ*, *kaṇu*, *kaṇṇu*, *kaṇpu* 'joint in knot or cane', *kaṇavu* 'node of bamboo, cane, etc.', *kaṇakkai*, *kaṇaṅkai* 'wrist', *kaṇakkāl*, *kaṇaṅkāl* 'ankle', *kaṇippu* 'articulation of limbs'; Kota *kaṇ* 'joint of bamboo'; Toda *koṇ* 'joint of bamboo or cane'; Kannaḍa *kaṇ* 'joint in reeds, sticks, etc.', *gaṇalu* 'knuckle of the fingers, joint or knot of any cane', *gaṇike* 'knot or joint'; Tuḷu *kāra kaṇṇu̥* 'ankle'; Telugu *kanu*, *kannu* 'joint in cane or reed', *kaṇupu*, *gaṇupu* 'joint, knot, node (of bamboo, sugarcane, etc.)'; Kolami *gana* 'knot in tree'; Naikṛi *khan* 'joint in bamboo'; Gondi *gana*, *ganakay* 'wrist'; Kuṛux *xann* 'place on bamboo or cane where side shoot was cut away'; Brahui *xan* 'knot in wood';
B. Proto-Indo-European (**k'en-*/**k'on-*/)**k'n-* 'knot, knob';
C. (?) Proto-Chukotian **kæŋkæl* 'tip of pole for driving reindeer'.

Note: Also found in Northwest Caucasian: cf. Proto-Circassian *_k’anə_ ‘knucklebone (used in bone game)’ > Bžedux _č”anə_, Kabardian _k’an_ ‘knucklebone (used in bone game)’.

15. Proto-Nostratic *_k’an_^{y}_-_ (~ *_k’ən_^{y}_-_) ‘(vb.) to observe, to perceive; (n.) that which observes, perceives: eye; perception, observation, recognition, comprehension’:

A. Proto-Afrasian *_k’an-_ ‘to observe, to perceive’;

B. Dravidian: Tamil _kaṇ_ ‘eye, aperture, orifice, star of a peacock’s tail’; Malayalam _kaṇ_, _kaṇṇu_ ‘eye, nipple, star in a peacock’s tail, bud’; Kota _kaṇ_ ‘eye’; Toda _koṇ_ ‘eye, loop in string’; Kannaḍa _kaṇ_ ‘eye, small hole, orifice’; Koḍagu _kaṇṇï_ ‘eye, small hole, orifice’; Tuḷu _kaṇṇu̥_ ‘eye, nipple, star in peacock’s feather, rent, tear’; Telugu _kanu_, _kannu_ ‘eye, small hole, orifice, mesh of net, eye of a peacock’s feather’; Kolami _kan_ ‘eye, small hole in ground, cave’; Naikṛi _kan_ ‘eye, spot in a peacock’s tail’; Naiki (of Chanda) _kan_ ‘eye’; Parji _kan_ ‘eye’; Gadba (Ollari) _kaṇ_ ‘eye’, (Salur) _kanu_ ‘eye’; Gondi _kan_ ‘eye’; Konḍa _kaṇ_ ‘eye’; Pengo _kaṇga_ ‘eye’; Manḍa _kan_ ‘eye’; Kui _kanu_ ‘eye’; Kuṛux _xann_ ‘eye, eye of a tuber’, _xannērnā_ ‘(of newly-born babies or animals) to begin to see, to have the use of one’s eyesight’; Malto _qanu_ ‘eye’; Brahui _xan_ ‘eye, bud’. Tamil _kāṇ_ (_kāṇp-_, _kaṇṭ-_) ‘(vb.) to see, to consider, to investigate, to appear, to become visible; (n.) sight, beauty’, _kāṇkai_ ‘knowledge’, _kāṇpu_ ‘seeing, sight’, _kaṇṇu_ (_kaṇṇi-_) ‘to purpose, to think, to consider’; Malayalam _kāṇuka_ ‘to see, to observe, to consider, to seem’, _kāṇikka_ ‘to show, to point out’; Kota _kaṇ-/ka·ṇ-_ (_kaḍ-_) ‘to see’; Toda _ko·ṇ-_ (_koḍ-_) ‘to see’; Kannaḍa _kāṇ_ (_kaṇḍ-_) ‘(vb.) to see, to appear; (n.) seeing, appearing’, _kāṇike_, _kāṇke_ ‘sight, vision, present, gift’, _kaṇi_ ‘sight, spectacle, ominous sight, divination’; Koḍagu _ka·ṇ-_ (_ka·mb-_, _kaṇḍ-_) ‘to see, to seem, to look’; Telugu _kanu_ (allomorph _kān-_), _kāncu_ ‘to see’; Kolami _kanḍt_, _kanḍakt_ ‘seen, visible’; Naikṛi _kank er-_ (< *_kanḍk-_ or the like) ‘to appear’; Parji _kanḍp-_ (_kanḍt-_) ‘to search, to seek’; Kuṛux _xannā_ ‘to be pleasant to the eye, to be of good effect, to suit well’; Brahui _xaning_ ‘to see’;

C. Proto-Indo-European *_k’en(H)-/*k’on(H)-/*k’n̥(H)-_, *_k’n-oH-_ (> *_k’nō-_) ‘to perceive, to recognize, to understand, to know’.

16. Proto-Nostratic *_k’ap_^{h}_-_ (~ *_k’əp_^{h}_-_) ‘jaw, jawbone’ (the Altaic cognates seem to point to Proto-Nostratic *_k’ep_^{h}_-_):

A. Dravidian: Tamil _kavuḷ_ ‘cheek, temple or jaw of elephant’; Malayalam _kaviḷ_ ‘cheek’; Tuḷu _kauḷu_ ‘the cheek’, _kavuṇḍrasa_, _kavuḍrasa_ ‘cancer of the cheek’; Parji _gavla_, (metathesis in) _galva_ ‘jaw’; (?) Telugu _gauda_ ‘the cheek’; (?) Kui _kūlu_ ‘cheek’;

B. Proto-Kartvelian *_(ni-)k’ap-_ ‘lower jaw, chin’;

C. Proto-Indo-European *_k’ep_^{h}_-/*k’op_^{h}_-_ ‘jaw, mouth’;

D. Proto-Altaic *_kēp_^{h}_a_ ‘jaw, face’.

17. Proto-Nostratic *_k’ap_^{h}_-_ (~ *_k’əp_^{h}_-_) ‘nape of the neck, back of the head’:

A. Proto-Afrasian *_k’ap-_ ‘nape of the neck, back of the head’;
B. Proto-Kartvelian *_k’ep-_ ‘nape of the neck, back of the head’;
C. Proto-Inuit *_kapəlʀuq_ or *_kapəlʀuk_ ‘neck part of an animal’.

18. Proto-Nostratic *_k’ar-_ (~ *_k’ər-_) ‘(vb.) to shout, to screech, to call (out to), to cry (out); (n.) call, cry, invocation, proclamation; roar, lamentation’:

A. Proto-Afrasian *_k’ar-_ ‘to call to’;
B. Dravidian: Tamil _karai_ (_-v-_, _-nt-_) ‘to sound, to roar, to weep, to lament, to call, to invite’, _karai_ (_-pp-_, _-tt-_) ‘to call, to summon’; etc.;
C. Proto-Indo-European *_k’er-_/*_k’or-_/*_k’r̥-_ ‘to call out to’.

19. Proto-Nostratic *_k’ar-_ ‘dark, dark-colored; dirty, soiled’:

A. Proto-Afrasian *_k’ar-_ ‘dark, dark-colored; dirty, soiled’;
B. Proto-Indo-European *_k’r-u-k’o-s_, _-eA_ [_-aA_] (> _-ā_) ‘dirt, grime’ > Greek (Hesychius) γρύξ· ‘dirt in the nails’; Modern English (regional) _crock_ ‘smut, soot, dirt’; Latvian _gruzis_ ‘dirt, smut; rubbish’;
C. Proto-Altaic *_karu_ (~ k^h-) ‘black’.

Note the parallel Proto-Nostratic stem *k^h_ar-_ ‘black, dark’, which is not related to the above.

Note Proto-North Caucasian *_k’ărV_ ‘black; coal’.

20. Proto-Nostratic *_k’ar-_ (~ *_k’ər-_) ‘(vb.) to twist, to turn, to bend, to wind; to tie (together), to bind; (adj.) curved, bent, crooked; tied, bound; (n.) that which is tied or bound together: bunch, bundle’:

A. Proto-Afrasian *_k’ar-_ ‘to twist, to turn, to bend, to wind; to tie (together), to bind’;
B. Dravidian: Kota _karv-_ (_kard-_) ‘to become tight (rope)’, _karv-_ (_kart-_) ‘to tighten (knot)’; Toda _kaṟ-_ (_kaṟθ-_) ‘to become tight’, _kaṟf-_ (_kaṟt-_) ‘to tighten (tr.)’. Tamil _kaṟṟai_ ‘collection (as of hair, rays of the sun), bundle (as of straw, grass, paddy seedlings), coconut leaves braided together like ropes as bands for hedging’; Malayalam _kaṟṟa_ ‘bundle (as of grass, straw), sheaf of corn’; Kannaḍa _kante_ ‘bundle (as of grass, straw, etc.)’;
C. Proto-Kartvelian *_k’ar-_/*_k’r-_ ‘to bind, to tie together’;
D. Proto-Indo-European *_k’er-_/*_k’or-_/*_k’r-_ ‘(vb.) to twist, to turn, to bend, to wind; to tie (together), to bind; (adj.) curved, bent, crooked; tied, bound; (n.) that which is tied or bound together: bunch, bundle’;
E. Proto-Finno-Ugrian *_kärɜ-_ ‘to twist or tie (together), to bind, to thread’;
F. Proto-Altaic *_kera-_ (~ -r^y-) ‘to bind, to wind around’.

21. Proto-Nostratic **k'ar-* (~ **k'ər-*) 'protuberance, lump, hump, breast' (possibly derived from **k'ar-* '[vb.] to twist, to turn, to bend, to wind; to tie [together], to bind; [adj.] curved, bent, crooked' in the sense 'curved shape, swelling'):

    A. Dravidian: *karaṭu* 'ankle, knot in wood'; Malayalam *karaṇa* 'knot of sugar-cane', *kuraṭṭa* 'knuckle of hand or foot'; Kannaḍa *karaṇe, kaṇṇe* 'clot, lump'; Telugu *karuḍu* 'lump, mass, clot';
    B. Proto-Kartvelian **m-k'erd-* 'breast, chest';
    C. Proto-Indo-European **k'er-/*k'or-/*k'r̥-* 'protuberance, lump, hump, breast' > Armenian *kurc* 'core, stump', (pl.) *kurckʰ* 'breasts'; Old Icelandic *kryppa* 'hump, hunch'; Lithuanian *grùbas* 'hump, lump, hillock'; Old Church Slavic *grudь* (< **grǫdь*) 'breast'; Russian *gorb* [горб] 'hump', *grud'* [грудь] 'breast, chest, bosom, bust'; Serbo-Croatian (pl.) *grudi* 'breasts'; Polish *garb* 'hump, lump'.

22. Proto-Nostratic **k'aw-* (~ **k'əw-*) '(vb.) to bend, twist, curve, or turn round; to rotate; (adj.) bent, curved, round; (n.) any round object':

    A. Proto-Afrasian **k'aw-* '(adj.) bent, curved, round; (n.) any round object: a hole';
    B. Dravidian: Tamil *kevi* 'deep valley, cave'; Kannaḍa *gavi* 'cave'; Tuḷu *gavi* 'cave, hole, cell'; Telugu *gavi* 'cavern';
    C. Proto-Kartvelian **k'w-er-*, (reduplicated) **k'wer-k'wer-* 'round object';
    D. Proto-Indo-European **k'ew-/*k'ow-/*k'u-*, also **k'ewH-/*k'owH-/*k'uH-* > **k'ū-* '(adj.) bent, curved, round; (n.) any round object';
    E. Proto-Chukotian **kawra-* 'to go round'.

23. Proto-Nostratic **k'aw-* (~ **k'əw-*) '(vb.) to take, to seize, to grasp, to hold; (n.) hand':

    A. Proto-Kartvelian **k'aw-/*k'w-* 'to take';
    B. Proto-Indo-European **k'ow(H)-/*k'u(H)-* (or **k'aw[H]-/*k'u[H]-*) '(vb.) to take, to seize, to grasp, to hold; (n.) hand'.

24. Proto-Nostratic (Eurasiatic only) **k'el-* 'female in-law; husband's sister':

    A. Proto-Indo-European **k'(a)lowV-*, **k'(a)lōC-* 'husband's sister';
    B. Proto-Altaic **kele* (~ *-i, -o*) 'daughter-in-law, bride'.

    Note: Not related to the parallel Proto-Nostratic stem **kʰal-* 'female in-law'.

25. Proto-Nostratic **k'enʸ-* 'knot, joint':

    A. Dravidian: Tamil *keṇṭai* 'ankle'; Kannaḍa *giṇṇu, geṇṇu* 'knot, joint (as of sugarcane, finger, etc.)', *gaṇṭu* 'knot of cord; joint of reed, bamboo, cane; joint or articulation of body'; Malayalam *keṇippu* 'joint, articulation'; Koḍagu *giṇṇï* 'joint in wrist or fingers, knot in sugarcane'; Tuḷu *gaṇṭu̥, gaṇṭu* 'knot in string, ankle, knot or joint of reed or cane'; Telugu *gaṇṭu*, *gaṇṭa* 'a knot'; Naikṛi *kaṇḍe* 'joint in bamboo';

B. Proto-Indo-European **k'enu-*/**k'nu-* (secondary *o*-grade form: **k'onu-*) 'knee, bend of the leg; angle';

C. Proto-Altaic **kēnʸa* 'front leg, armpit, angle'.

26. Proto-Nostratic **k'ep'-* '(vb.) to cut, chop, split, or break into small pieces; to munch, to chew; (n.) the act of cutting, chopping, splitting, or breaking into small pieces, the act of mincing; chewing (the cud), rumination':

A. Afrasian: Semitic: Arabic *ḳabaʔa* 'to eat, to fill oneself with drink';
B. Proto-Kartvelian **k'ep'-* 'to cut or chop into small pieces, to mince';
C. Proto-Altaic **kēpu-* 'to chew'.

27. Proto-Nostratic (Eurasiatic only) **k'er-* '(vb.) to decay, to wear out, to wither, to waste away, to become old; (adj.) decayed, worn out, withered, wasted, old':

A. Proto-Indo-European **k'er(H)-*/**k'or(H)-*/**k'r̥(H)-* 'to decay, to wear out, to wither, to waste away, to become old';
B. Proto-Altaic **kĕru* (~ *kʰ-*) 'old, worn out'.

28. Proto-Nostratic **k'er-* '(vb.) to gather, to collect; to take a handful, to pick, to pluck; (n.) collection, gathering, handful':

A. Proto-Afrasian **k'[e]r-* 'to gather, to collect; to take a handful, to pick, to pluck';
B. Dravidian: Koṇḍa *ker-* 'to take handfuls or small quantities out of a mass (of grain, etc.), to take into a ladle before serving, to collect into a heap and pick up'; Pengo *gre-* 'to scoop up with the hand'; Maṇḍa *grepa-* 'to scoop up'; Kui *grāpa* (*grāt-*), *grēpa* (*grēt-*) 'to scoop up, to shovel into with the hands, to scrape together'; Kuwi *grecali* (*gret-*) 'to gather up, to take a handful';
C. Proto-Kartvelian **k'er-b-*/**k'r-eb-* 'to gather, to collect', **k'r-ep-*/**k'r-ip-* 'to gather, to pick (fruit, flowers)'; perhaps also Georgian *k'ert'-*/*k'rt'-* 'to pluck (out)';
D. Proto-Indo-European **k'er-*/**k'or-*/**k'r̥-* 'to gather (together), to collect, to take a handful';
E. Uralic: Finnish *kerätä-* 'to collect, to gather together, to gather up; to pick', *keruu* 'collection, gathering', *keräys* 'collection', *kertyä-* 'to accumulate, to pile up', *keräätyä-* 'to collect, to gather; to assemble'; Karelian *kereä-* 'to gather, to collect'.

29. Proto-Nostratic **k'ir-* (~ **k'er-*) or **k'ur-* (~ **k'or-*) 'to cut, to cut into, to incise, to engrave, to notch; to cut off, to sever, to nip off, to clip; to cut in two, to split':

A. Proto-Afrasian **k'e(e)r-*, **k'o(o)r-* 'to cut, to cut into, to incise, to engrave, to notch; to cut off, to sever, to nip off, to clip; to cut in two, to split';

B. Proto-Kartvelian *_k’r-eč’k’-_/*_k’r-ič’k’-_/*_k’r̥-č’k’-_ ‘to cut, to cut off’;
C. Proto-Indo-European *_k’er-_/*_k’or-_/*_k’r̥-_ (extended form *_k’er-b_^{h}_-_/*_k’or-b_^{h}_-_/*_k’r̥-b_^{h}_-_) ‘to cut, to carve, to notch’;
D. Proto-Altaic *_kiro-_ ‘to cut, to mince’.

Note Proto-North-Caucasian *_k’irV_ ‘knife, axe’.

30. Proto-Nostratic *_k’os-_ ‘bone’:

A. Proto-Afrasian *_k’os-_ (~ *_k’as-_) ‘bone’;
B. Proto-Dravidian *_kōcc-_ ‘bone’: Kuṛux _xōcol_ ‘bone’; Malto _qoclu_ ‘bone’;
C. (?) Proto-Indo-European *_k_^{h}_os-t_^{h}_-_ (if from *_k’os-t_^{h}_-_) ‘rib, bone’.

Note: The putative Mordvin cognates cited by Illič-Svityč [1971— .I:344, no. 219] do not belong here — they go back to Proto-Finno-Permian *_kaskɜ_ ‘sacral region, lumbar region, small of the back’. This is one of the small number of examples that appear to support the Moscovite position.

31. Proto-Nostratic *_k’ud-_ ‘hind-part, end, tail’:

A. Afrasian: Highland East Cushitic: Burji _k’ud-ee_ (adv.) ‘in back of, behind’ (< ‘hind-part, back, end’);
B. Dravidian: Tamil _kūti_ ‘pudendum muliebre’; Malayalam _kūti_ ‘posteriors, membrum muliebre’; Toda _ku·θy_ ‘anus, region of the buttocks in general’; Tuḷu _kūdi_ ‘anus, posteriors, membrum muliebre’;
C. Proto-Kartvelian *_k’ud-_ ‘tail’.

32. Proto-Nostratic *_k’ud-_ (~ *_k’od-_) ‘(vb.) to strike; (n.) stroke, blow, knock, cuff, thump’:

A. Dravidian: Tamil _kuṭṭu_ (_kuṭṭi-_) ‘to cuff, to strike with the knuckles on the head or temple’; Malayalam _kuṭṭuka_ ‘to pound, to cuff’; Kota _kuṭ-_ (_kuc-_) ‘to pound’; etc. Tamil _koṭṭu_ (_koṭṭi-_) ‘(vb.) to beat (as a drum, tambourine), to hammer, to beat (as a brazier), to clap, to strike with the palms, to pound (as paddy); (n.) beat, stroke, drumbeat, time-measure’, _koṭṭāṉ_, _koṭṭaṉ_ ‘mallet’, _koṭu_ ‘to thrash, to abuse roundly’, _koṭai_ ‘blows, round abuse’; etc. (Either here or with *_k’_^{w}_ad-_ ‘[vb.] to strike, to beat, to smash, to pound; [n.] knock, stroke, thrust’ [see below]);
B. Proto-Kartvelian *_k’od-_ ‘to hew, to hollow’, *_k’od-al-_ ‘wood-pecker’.

33. Proto-Nostratic *_k’ud-_ (~ *_k’od-_) ‘vessel, pot’:

A. Proto-Afrasian *_k’od-_ ‘vessel, pot’;
B. Dravidian: Tamil _kuṭam_ ‘waterpot, hub of a wheel’, _kuṭaṅkar_ ‘waterpot’, _kuṭantam_ ‘pot’, _kuṭukkai_ ‘coconut or hard shell used as a vessel, pitcher’, _kuṭikai_

'ascetic's pitcher', *kuṭuvai* 'vessel with a small narrow mouth, pitcher of an ascetic'; etc.;

C. Proto-Kartvelian **k'od-* 'vessel carved from a single piece of wood'.

34. Proto-Nostratic **k'ul*^{y}*-* (~ **k'ol*^{y}*-*) '(vb.) to be or become cold; to freeze; (n.) cold, coldness, chill, frost':

A. Dravidian: Tamil *kuḷircci*, *kuḷirtti*, *kuḷutti* 'coldness, cold, act of cooling or refreshing, numbness', *kuḷir* '(vb.) to feel cool; to be cool, refreshing; to get numbed; (n.) coldness, chilliness, ague, shivering', *kuḷirppu*, *kuḷirmai*, *kuḷumai* 'coolness, kindness', *kuḷir* 'a fan', (reduplicated) *kuḷḷa-kkuḷir-* 'to be intensely cool and refreshing'; Malayalam *kuḷir*, *kuḷur* 'coldness; cool, refreshing', *kuḷiruka* 'to be chilly, refreshed', *kuḷirma* 'freshness', *kuḷirppu*, *kuḷuppam* 'chilliness', *kuḷirppikka* 'to chill, to quiet, to refresh, to comfort', (reduplicated) *kuḷukuḷu* 'intense cold'; Kota *kuḷak in-*, (reduplicated) *kuḷkuḷ in-* '(hands, feet, body) to feel cool, (mind) to feel calm and peaceful'; Kannaḍa *kuḷir* '(vb.) to be cool or cold; (n.) coldness, coolness, cold, snow, frost'; Koḍagu *kuḷï-* (*kuḷïp-*, *kuḷït-*) 'to feel cold', *kuḷïrï ka·la* 'cold season';

B. Proto-Indo-European **k'ol-*/**k'l̥-* (secondary *e*-grade form: **k'el-*) '(vb.) to be or become cold; to freeze; (n.) cold, coldness, chill, frost';

C. Proto-Finno-Permian **külmä* (**kilmä*) '(adj.) cold, chilly; (n.) frost; (vb.) to become cold, to freeze';

D. Proto-Altaic **kol*^{y}*i-* (~ *k*^{h}-; *-i̯-*, *-e-*) 'to freeze'.

35. Proto-Nostratic **k'um-* (~ **k'om-*) '(vb.) to sigh, to weep, to lament, to moan, to groan; (n.) sigh, mourning, lamentation, moan, groan, roar, grumble':

A. Proto-Afrasian **k'um-* 'to sigh, to weep, to lament, to moan, to groan';

B. Dravidian: Tamil *kumuṟu* (*kumuṟi-*) 'to resound, to trumpet, to bellow, to crash (as thunder), to have confused uproar', *kumuṟal* 'roaring, resounding', *kumiṟu* (*kumiṟi-*) 'to resound, to roar'; Malayalam *kumuṟuka* 'to make thundering sound';

C. Proto-Kartvelian **k'um-in-* 'to moan, to grumble';

D. Proto-Indo-European **k'om-*/**k'm-* (secondary *e*-grade form: **k'em-*) 'to sigh, to weep, to lament, to moan, to groan';

E. (?) Proto-Chukotian **kumŋə(kum)* 'voice, sound'.

36. Proto-Nostratic **k'um-* (~ **k'om-*) '(vb.) to press together; (n.) heap, mass, lump, clump; pressure, compression':

A. Proto-Afrasian **k'[u]m-* 'to press together; to seize, to grasp';

B. Proto-Kartvelian **k'um-* 'to press together';

C. Proto-Indo-European **k'om-*/**k'm-* (secondary *e*-grade form: **k'em-*) 'to press together; to seize, to grasp'.

37. Proto-Nostratic **k'un-* (~ **k'on-*) '(vb.) to bend; to bend or fold (together); to tie or bind together; (n.) that which is bent, folded, crooked, curved, hooked: bend, fold, curve, curvature, angle, wrinkle':

A. Proto-Afrasian **k'[u]n-* 'to bend': Semitic: Arabic *ḳaniya* 'to be hooked, aquiline (nose)', *ʔaḳnā* 'bend, curved, crooked, hooked'. Egyptian *qnb* 'to bend, to bow, to incline (oneself); to subjugate', *qnbt* 'corner, angle', *qnỉ* 'sheaf, bundle'; Coptic *knaaw* [ⲕⲛⲁⲁⲩ] (< *qnỉw*) 'sheaf';
B. Dravidian: Tamil *kūn̲* 'bend, curve, hump on the back, humpback, snail', *kūn̲u* (*kūn̲i-*) 'to curve, to become crooked, to bend down, to become hunchbacked', *kūn̲al* 'bend, curve, hump', *kūn̲an* 'humpback', *kūn̲i* (*-v-*, *-nt-*) 'to bend (as a bow), to bow, to stoop'; (*-pp-*, *-tt-*) 'to bend (tr.), to stoop', *kūn̲i* 'curvature, bow (weapon)'; Malayalam *kūnuka* 'to stoop, to be crookbacked', *kuni* 'semicircle, curve', *kuniyuka* 'to bow, to stoop, to bend', *kunikka* 'to make a curve, to cause to stop stooping'; Kannaḍa *kūn* (*kūnt-*), *kūnu* 'to bend, to stoop, to crouch, to contract oneself, to shrivel up'; Koḍagu *kūn* 'hunchback'; Tuḷu *gūnu* 'a hump'; Telugu *gūnu* 'a hump, a crooked back'; Gondi *gun-* 'to bend';
C. Proto-Kartvelian **k'on-* 'to tie together';
D. Proto-Altaic **kunu-* (~ k^h-) 'to fold, to twist'.

38. Proto-Nostratic **k'uŋ-* (~ **k'oŋ-*) 'buttocks, rump, anus':

A. Dravidian: Tamil *kuṇṭi* 'buttocks, rump; bottom (as of a vessel), end of a fruit or nut opposite to the stalk'; Malayalam *kuṇṭi* 'posterior, anus; bottom (of a vessel)'; Kannaḍa *kuṇḍe* 'buttocks, anus; bottom (of a vessel)'; Telugu *kuṭṭe* 'anus'; Gadba *kuṇḍ* 'anus'; Kuwi *kūna* 'buttock';
B. Proto-Indo-European **k'un-k'o-s* 'rump, buttocks': Czech *huzo* 'rump, buttocks'; Slovenian *gǫ́za* 'rump, buttocks'; Old Polish *gąz* 'protuberance, hump' (Modern Polish *guz* 'lump', *guza* 'posterior'); Russian *guz* [гуз] 'rump, buttocks', *gúzka* [гузка] 'rump (of a bird)', *gúzno* [гузно] (vulgar) 'ass, bum';
C. Proto-Altaic **kuŋ*t^h*V* (~ *-o-*) 'rump, anus'.

39. Proto-Nostratic **k'ur-* (~ **k'or-*) or **k'ar-* (~ **k'ər-*) 'crane':

A. Dravidian: Tamil *kokku* (< **kor-kku* < **korV-nk-/-nkk-*) 'common crane', *kuruku* 'heron, stork, crane, bird, gallinaceous fowl'; Malayalam *kokku*, *kokkan*, *kocca*, *kuriyan* 'paddy bird, heron', *kuru* 'heron'; Kannaḍa *kokku*, *kokkare* 'crane', *kukku* 'heron, crane; Telugu *koṅga*, *kokkera*, *kokkarāyi* 'crane'; Kolami *koŋga* 'crane'; Tuḷu *korṅgu* 'crane, stork'; Parji *kokkal* 'crane'; Gondi *koruku* 'crane';
B. Proto-Indo-European **k'er-/*k'or-/*k'r-* 'crane';
C. Proto-Uralic **korkз* (~ **karke*) 'crane'.

40. Proto-Nostratic **k'ut'-* 'short, small':

A. Proto-Afrasian **k'ut'-* 'short, small';

B. Dravidian: Tamil *kuṭṭam* 'smallness, young of a monkey', *kuṭṭan* 'laddie, lassie (as a term of endearment)', *kuṭṭi* 'young of a dog, pig, tiger, etc.; little girl; smallness', *kuṭṭai* 'shortness, dwarfishness'; Malayalam *kuṭṭan* 'boy, lamb, calf', *kuṭṭi* 'young of any animal, child (chiefly girl); pupil of eye', *kuṭu* 'small, narrow'; Kota *kuṭ* 'short, small'; Kannaḍa *giḍḍu, guḍḍu* 'shortness, smallness', *giḍḍa* 'dwarf', *guḍḍa* 'dwarf, a boy; smallness, shortness'; Koḍagu *kuṭṭi* 'child of any caste except Coorgs, young of animals (except dog, cat, pig)'; Tuḷu *giḍḍa* 'small, short'; Telugu *giḍḍa, giḍaka* 'short, dwarfish', *guḍḍa* 'child'; Kui *gūṭa* 'short, dwarfish', *gūṭi* 'stumpy, short, shortened'; Kuṛux *guḍrū, guṛṛū* 'dwarfish (of persons and animals only)'; Brahui *ghuḍḍū, guḍḍū* 'small, urchin';

C. Proto-Kartvelian **k'ut'-* 'little, small';

D. Proto-Altaic **ki̯ūta* (~ *-tʰ-*) 'insufficiency, debt': Proto-Tungus **kōta* 'debt; miserly, greedy' > Evenki *kōta* 'debt'; Lamut / Even *qōt* 'debt'; Ulch *qota* 'miserly, greedy'; Orok *quta* 'miserly, greedy'; Nanay / Gold *qota* 'miserly, greedy'. Proto-Turkic **Kït-* 'not enough, insufficient' > Turkish *kıt* 'little, few, scarce, deficient', *kıtlaş-* 'to become scarce', *kıtlık* 'scarcity, dearth, famine', *kıtıpiyos* 'common, poor, trifling, insignificant'; Azerbaijani *ɢït* 'not enough, insufficient'; Turkmenian *ɢït* 'not enough, insufficient'; Uighur *qitiɣir* 'miserly'; Karaim *qït* 'not enough, insufficient'; Bashkir (dial.) *qïtlïq* 'hunger'; Kirghiz *qïdïq* 'dwarf', *qïtïy-* 'secretive', *qïtïray-* 'lean and small'; Kazakh *qïtïqtan-* 'to be offended'; Noghay *qït* 'not enough, insufficient'; Chuvash *χədəχ* 'compulsion'; Tuva *qïdïɣ* 'oppressed'.

Proto-Nostratic *k'ʷ:

1. Proto-Nostratic **k'ʷad-* (~ **k'ʷəd-*) '(vb.) to strike, to beat, to smash, to pound; (n.) knock, stroke, thrust':

A. Dravidian: Tamil *kuṭṭu* (*kuṭṭi-*) 'to cuff, to strike with the knuckles on the head or temple'; Malayalam *kuṭṭuka* 'to pound, to cuff'; Kota *kuṭ-* (*kuc-*) 'to pound'; etc. Tamil *koṭṭu* (*koṭṭi-*) '(vb.) to beat (as a drum, tambourine), to hammer, to beat (as a brazier), to clap, to strike with the palms, to pound (as paddy); (n.) beat, stroke, drumbeat, time-measure', *koṭṭāṉ, koṭṭaṉ* 'mallet', *koṭu* 'to thrash, to abuse roundly', *koṭai* 'blows, round abuse'; etc. (Either here or with **k'ud-* '[vb.] to strike; [n.] stroke, blow, knock, cuff, thump' [see above]);

B. Proto-Indo-European **k'ʷedʰ-*/**k'ʷodʰ-* 'to strike, to beat, to smash';

C. Proto-Eskimo **kaðuɣ-* 'to strike (with an instrument)'.

2. Proto-Nostratic **k'ʷaħ-* (~ **k'ʷəħ-*) '(vb.) to hit, to strike, to beat, to pound; to push or press in; (adj.) hit, beaten, pounded, pushed or pressed together, crammed, filled':

A. Proto-Afrasian **k'ʷaħ-* 'to hit, to strike, to beat, to pound; to push or press in';

B. Proto-Kartvelian **k'wex-* 'to push in, to fill in';

C. Proto-Indo-European **k'ʷeħ͜h-dʰ-* [**k'ʷaħ͜h-dʰ-*] (> **k'ʷādʰ-*) 'to push or press in, to dive or plunge into'.

3. Proto-Nostratic **k'ʷal-* (~ **k'ʷəl-*) '(vb.) to go: to go away from, to go after or behind; (n.) track, way':

   A. Afrasian: Proto-Southern Cushitic **k'ʷaal-* 'to come from';
   B. Proto-Kartvelian **k'wal-* 'track, trace';
   C. Indo-European: Tocharian A *kälk-*, *kalk-* used to form the non-present tenses of *i-* 'to go', B *kālak-* 'to follow'. Assuming development from Proto-Indo-European **k'ʷel-*/**k'ʷol-*/**k'ʷl̥-* 'to go, to follow', attested only in Tocharian.

4. Proto-Nostratic **k'ʷam-* (~ **k'ʷəm-*) '(vb.) to burn slowly, to smolder; to be hot, to be red-hot, to be glowing; to smoke; (n.) embers, ashes; heat; smoke':

   A. Afrasian: Semitic: Akkadian *ḳamū* 'to burn, to consume by fire';
   B. Dravidian: Tamil *kumpu* (*kumpi-*) 'to become charred (as food when boiled with insufficient fire)', *kumai* 'to be hot, sultry'; Malayalam *kumpal* 'inward heat', *kummu* expression descriptive of heat, *kumuṟuka*, *kumiṟuka* 'to be hot, close', *kumuṟal* 'oppressive heat'; Kannaḍa *kome* 'to begin to burn (as fire or anger)'; etc.
   C. Proto-Kartvelian **k'wam-*/**k'wm-* 'to smoke';
   D. Uralic: Proto-Finno-Volgaic **kūma* 'hot, red-hot; fever'.

5. Proto-Nostratic **k'ʷar-* (~ **k'ʷər-*) '(vb.) to be cold; (n.) cold, coldness':

   A. Proto-Afrasian **k'ʷar-* (~ **k'ʷor-*) 'to be cold';
   B. Dravidian: Kannaḍa *koṟe*, *koṟi* 'to pierce (as cold)', *koṟeta*, *koṟata* 'the piercing of cold'; Kota *korv-* (*kord-*) 'to be cold', *kor*, *korv* 'coldness'; Gondi *kharrā* 'frost', *karīng*, *koring* 'cold'; Toda *kwaṟ-* (*kwaṟθ-*) 'to feel cold', *kwaṟ* 'cold', *kwar-* (*kwarθ-*) 'to be cold (in songs)'; Kolami *korale* 'cold';
   C. Kartvelian: Georgian (Lečxumian) *k'rux-wa* 'cold'; Svan *k'warem* 'ice', *k'warmob* 'frost, freezing', *lik'wremi* 'to freeze'.

6. Proto-Nostratic **k'ʷar-* (~ **k'ʷər-*) '(vb.) to rest, to stay, to remain; (adj.) still, quiet, at rest; (n.) stillness, quietude, repose, rest, resting place':

   A. Proto-Afrasian **k'ʷar-* 'to stay, to remain, to rest, to settle down';
   C. (?) Dravidian: Kannaḍa *kūr* 'to sit down', *kūrisu* 'to cause to sit'; Telugu *kūr(u)cuṇḍu* 'to sit, to be seated'; Pengo *kuc-* 'to sit'; Manḍa *kuh-* 'to sit';
   D. Proto-Indo-European **k'ʷer-*/**k'ʷor-*/**k'ʷr̥-* 'gentle, mild, calm, at rest, still'.

7. Proto-Nostratic **k'ʷar-* (~ **k'ʷər-*) '(vb.) to crush, to grind; (n.) grinding pestle, grinding stone; stone, rock':

   A. Dravidian: Tamil *kur̤avi* 'grinding pestle'; Malayalam *kur̤avi* 'small rolling stone to grind with'. Tamil *kuṟu* (*kuṟuv-*, *kuṟṟ-*) 'to pound in a mortar, to husk', *kuṟṟu*

(*kuṟṟi-*) ‘to pound, to strike, to hit, to crush’; Kota *kur-* (*kuṯ-*) ‘to pound (clay in preparation for making pots)’; Gadba *kurk-* (*kuruk-*) ‘to beat like a carpet’; Gondi *kurkal* ‘stone pestle’;
B. Proto-Kartvelian **k’werčx-* ‘to break up, to split, to crush, to smash’;
C. Proto-Indo-European **k’ʷerAn-/*k’ʷr̥An-*, **k’ʷreAn-* [**k’ʷraAn-*] (> **k’ʷrān-*), **k’ʷreAwn̥-* [**k’ʷraAwn̥-*] (> **k’ʷrāwn̥-*) ‘mill, millstone’.

8. Proto-Nostratic **k’ʷar-bV-* (~ **k’ʷər-bV-*) ‘the inside, the middle, interior, inward part’:

A. Proto-Afrasian **k’ʷarb-* ‘the inside, the middle, interior, inward part’;
B. Dravidian: Tamil *karu* ‘fetus, embryo, egg, germ, young of animal’, *karuppai* ‘womb’, *karuvam* ‘fetus, embryo’; Malayalam *karu* ‘embryo, yolk’; Kota *karv* ‘fetus of animal, larva of bees, pregnant (of animals)’; Telugu *karuvu* ‘fetus’, *kari* ‘uterus of animals’; Parji *kerba* ‘egg’; Gadba (Ollari) *karbe* ‘egg’; Gondi *garba* ‘egg’;
C. Proto-Indo-European **k’ʷerbʰ-/*k’ʷorbʰ-*, **k’ʷrebʰ-* ‘the inside, the middle, interior, inward part’.

9. Proto-Nostratic **k’ʷas-* (~ **k’ʷəs-*) ‘(vb.) to strike fire, to put out (fire); (n.) spark, fire’:

A. Afrasian: Proto-Semitic (reduplicated) **k’as-k’as-* ‘to stroke or stir up (a fire)’ > Geez / Ethiopic *ḳʷasḳʷasa* ‘to stir a fire’; etc.;
B. Dravidian: Konḍa *kas-* ‘to be lit (as fire), to burn’, *kasis-* ‘to light (lamp, fire)’; Pengo *kacay ki-* ‘to light (lamp)’; Kuwi *hiccu kahinomi* ‘we kindle fire’;
C. Proto-Kartvelian **k’wes-* ‘to strike fire’;
D. Proto-Indo-European **k’ʷes-/*k’ʷos-* ‘to extinguish, to put out (originally, of fire)’.

Note: Also found in Northwest Caucasian: cf. Proto-Circassian **k’ᵒasa* ‘to go out (as fire, light); to escape, to run away, to desert, to elope’ > Bžedux *k’ᵒāsa*, Kabardian *k’ᵒāsa* ‘to go out (as fire, light); to escape, to run away, to desert, to elope’.

10. Proto-Nostratic **k’ʷas-* (~ **k’ʷəs-*) ‘(vb.) to sigh, to moan, to groan; to whisper, to murmur, to mumble; (n.) sigh, moan, groan, whisper, murmur, mumble’ (onomatopoeic):

A. Proto-Afrasian **k’ʷas-* ‘to sigh, to moan, to groan; to whisper, to murmur, to mumble’;
B. Dravidian: Tamil (reduplicated) *kucukucu* (*-pp-*, *-tt-*) ‘to whisper’, *kucukucuppu* ‘whispering’, *kacu-kuc-eṉal* onomatopoeic expression signifying whispering; Malayalam *kuśukuśukka*, *kucukucukka* ‘to whisper’, *kuśalikka* ‘to whisper, to mumble’, *kaśukuśu* imitative sound of whispering; etc.;

C. Proto-Kartvelian **k'wes-/*k'ws-* 'to moan';
D. Indo-European: Old Icelandic *kvis* 'rumor, tattle', *kvisa* 'to gossip, to whisper'; Norwegian *kvisa* 'to whisper'; Swedish (dial.) *kvisa* 'to whisper'; Low German *quesen* 'to grumble'; New High German (dial.) *queisen* 'to sigh, to moan, to groan'.

11. Proto-Nostratic **k'ʷat'-* (~ **k'ʷət'-*) '(vb.) to burn, to smolder, to smoke; (n.) burning, heat, smoke':

A. Proto-Afrasian **k'ʷat'-* (vb.) to burn, to smolder, to smoke; (n.) smoke';
B. Dravidian: Malayalam *kattuka* 'to kindle, to burn', *kattal* 'burning, heat, appetite', *kattikka* 'to set on fire, to burn'; Kota *kat-* (*katy-*) 'to burn (intr.), to light (lamp)', *katc-* (*katc-*) 'to set fire to'; etc.;
C. Proto-Indo-European **k'ʷət'-/*k'ʷat'-* > (with regressive deglottalization) *kʷʰet'-/*kʷʰot'-* '(vb.) to burn, to smoke, to smolder; (n.) smoke'.

12. Proto-Nostratic **k'ʷat'-* (~ **k'ʷət'-*) '(vb.) to cut; (n.) knife, cutting instrument; (adj.) sharp':

A. Proto-Afrasian **k'ʷat'-* 'to cut';
B. Dravidian: Malayalam *katti* 'knife'; Kota *katy* 'billhook, knife', *kati·r* 'to cut'; Tamil *katti* 'knife, cutting instrument, razor, sword, sickle';Kannaḍa *katti* 'knife, razor, sword'; etc. Kolami *katk-* (*katakt-*) 'to strike down (man), to break down (tree)'; Naiki (of Chanda) *katuk-/katk-* 'to cut with an axe'; Parji *katt-* 'to cut down (tree), to slaughter, to sacrifice'; etc.;
C. Proto-Kartvelian **k'wet'-* > (with progressive deglottalization) **k'wet-/*k'wt-* 'to chop, to cut off';
D. Proto-Indo-European **k'ʷət'-/*k'ʷat'-* > (with regressive deglottalization) **kʷʰet'-/*kʷʰot'-* 'to whet, to sharpen'.

13. Proto-Nostratic **k'ʷed-* '(vb.) to destroy, to damage, to ruin; to decay, to rot, to spoil; (n.) death, destruction, damage, ruin, decay':

A. Dravidian: Tamil *keṭu* (*keṭuv-*, *keṭṭ-*) 'to perish, to be destroyed, to decay, to rot, to become damaged, to become spoiled, to fall on evil days, to degenerate, to be reduced, to run away defeated', *keṭu* (*-pp-*, *-tt-*) '(vb.) to destroy, to squander, to extinguish, to spoil, to corrupt, to defeat, to lose; (n.) peril, poverty', *keṭṭa* 'bad, spoiled, ruined', *keṭṭavaṉ* 'a bad, immoral person', *keṭutal* 'ruin, damage, danger, degeneracy', *keṭuti* 'ruin, loss, damage, thing lost, danger, affliction, evil', *keṭumpu* 'ruin, evil', *kēṭu* 'ruin, loss, damage, adversity, death, evil'; etc.;
B. Proto-Kartvelian **k'wed-/*k'wd-* '(vb.) to die, to lose; (n.) death, loss';
C. Proto-Indo-European **k'ʷĕdʰ-/*k'ʷŏdʰ-* 'rotten, bad, repulsive'.

Note: Perhaps also found in Northwest Caucasian: cf. Proto-Circassian **k'ᵒad(a)* 'to disappear, to get lost, to perish' > Bžedux *k'ᵒadə*, Kabardian *k'ᵒad* 'to disappear, to get lost, to perish'.

14. Proto-Nostratic **k'ʷiy-* (~ **k'ʷey-*) '(vb.) to be putrid, purulent; (n.) pus':

A. Afrasian: Proto-Semitic **k'ay-aħ-* 'to fester, to be purulent' > Arabic *ḳāḥa* 'to fester, to be purulent', *ḳayḥ* (pl. *ḳuyūḥ*) 'pus, mucous matter';
B. Dravidian: Tamil *cī* 'pus, mucous matter'; Malayalam *cī* 'putrid matter, secretion of the eyelids'; Kannaḍa *kī* 'to become pus, to become putrid'; Koḍagu *ki·y-* (*ki·yuv-*, *ki·ñj-*) 'to become rotten'; Telugu *cīku* 'to rot', *cīmu* 'pus';
C. Proto-Indo-European **k'ʷey-/*k'ʷi-* 'to be putrid, purulent'.

15. Proto-Nostratic **k'ʷow-* 'bullock, ox, cow':

A. Dravidian: Telugu *kōḍiya*, *kōḍe* 'young bull'; Kolami *kōḍi* 'cow', *kōṛe* 'young bullock'; Pengo *kōḍi* 'cow'; Manḍa *kūḍi* 'cow'; Kui *kōḍi* 'cow, ox'; Kuwi *kōdi*, *kōḍi* 'cow';
B. Proto-Indo-European **k'ʷŏ̄w-* 'bullock, ox, cow'.

16. Proto-Nostratic (Eurasiatic only) **k'ʷoy-* 'outer covering: skin, hide, leather; bark (of a tree), shell, crust':

A. Proto-Indo-European **k'ʷoyH-/*k'ʷiH-* (secondary *e*-grade form: **k'ʷeyH-*) 'skin, hide, leather';
B. Uralic: Proto-Finno-Ugrian **koya* 'outer covering: skin, hide, leather; bark (of a tree), shell, crust'.

17. Proto-Nostratic **k'ʷurʸ-* (~ **k'ʷorʸ-*) '(to be) heavy, weighty, solid, bulky':

A. Proto-Afrasian **k'ʷur-* 'to be heavy, weighty';
B. Dravidian: Tamil *kor̤u* '(adj.) fat, flourishing, prosperous; (n.) fat; (vb.) to prosper, to flourish, to be rich or fertile (as soil), to grow fat, to be plump, to be of thick consistency (as sandal paste), to be saucy, to be insolent', *kor̤umai* 'plumpness, luxuriance, thickness, fertility', *kor̤uppu* 'richness, fat, grease, plumpness, thickness in consistency, sauciness, impudence'; Malayalam *kor̤ukka* 'to grow thick, solid, stiff by boiling; to grow fat, stout, arrogant', *kor̤uppu* 'solidity (as of broth or curry), fatness, stoutness, pride', *kor̤u* 'fat, thick, solid'; etc.;
C. Proto-Indo-European **k'ʷor(H)-/*k'ʷr̥(H)-* (secondary *e*-grade form: **k'ʷer(H)-*) 'heavy, weighty'.

Proto-Nostratic *q':

1. Proto-Nostratic **q'ab-* (~ **q'əb-*) 'jaw':

   A. (?) Dravidian: Tamil _kavuḷ_ ‘cheek, temple or jaw of elephant’; Malayalam _kaviḷ_ ‘cheek’; Tuḷu _kauḷu_ ‘the cheek’, _kavuṇḍrasa_, _kavuḍrasa_ ‘cancer of the cheek’; Parji _gavla_, (metathesis in) _galva_ ‘jaw’; (?) Telugu _gauda_ ‘the cheek’; (?) Kui _kūlu_ ‘cheek’ — either here or with Proto-Nostratic _*k’ap_^{h}_-_ (~ _*k’əp_^{h}_-_) ‘jaw, jawbone’;
   B. Proto-Kartvelian _*q’ab-_ ‘jaw’;
   C. Proto-Indo-European _*k’eb_^{h}_-/*k’ob_^{h}_-_ ‘(vb.) to munch, to chew; (n.) jaw’.

2. Proto-Nostratic _*q’al_^{y}_-_ (~ _*q’əl_^{y}_-_) ‘sexual organs, genitals, private parts (male or female)’:

   A. Afrasian: Semitic: Akkadian _ḳallū_, _gallū_ ‘sexual organ’ (this is usually thought to be a loan from Sumerian); Geez / Ethiopic _ḳ_^{w}_əlḥ_ ‘testicle’; Amharic _ḳ_^{w}_əla_ ‘testicle’;
   B. Proto-Kartvelian _*q’al-_ ‘penis’;
   C. Proto-Indo-European _*k’el-t_^{h}_-/*k’l̥-t_^{h}_-_ ‘vulva, womb’;
   D. Proto-Finno-Permian _*kal_^{y}_kkз_ ‘egg, testicle’;
   E. Proto-Chukchi-Kamchatkan _*qəlqæ_ ‘penis’.

3. Proto-Nostratic _*q’am-_ (~ _*q’əm-_) ‘to crush, to grind; to chew, to bite, to eat’:

   A. Proto-Afrasian _*k’am-_ ‘(vb.) to crush, to grind; to chew, to bite, to eat; (n.) flour’;
   B. Proto-Indo-European _*k’em-b_^{h}_-/*k’om-b_^{h}_-/*k’m̥-b_^{h}_-_ ‘to chew (up), to bite, to cut to pieces, to crush’, _*k’om-b_^{h}_o-s_ ‘tooth, spike, nail’;
   C. Proto-Chukotian _*qametva-_ (or _*qamatva-_) ‘to eat’.

4. Proto-Nostratic _*q’an-_ (~ _*q’ən-_) ‘field, land, (open) country’:

   A. (?) Afrasian: Egyptian _qn_ used as a designation for plants in a field, _qnt_ ‘plant’, _qnỉ_ ‘plant’;
   B. Proto-Kartvelian _*q’an-_ ‘cornfield, plowed field’;
   C. Proto-Finno-Permian _*kentä_ ‘field, meadow, pasture’.

5. Proto-Nostratic _*q’ar_^{y}_-_ (~ _*q’ər_^{y}_-_) ‘(vb.) to rot, to stink; (n.) rotten, stinking, putrid’:

   A. Dravidian: Gondi _kaṛītānā_ ‘to be rotten, to rot, to decay’, _kaṛi-_ ‘to be rotten, to go rotten’, _kaṛīstānā_ ‘to rot, to ret (hemp)’; Koṇḍa _kaṛk-_ ‘to go bad, to become rotten’; Pengo _kṛaŋ(g)-_ (_kṛaŋt-_) ‘to go bad, to become rotten (egg)’;
   B. Proto-Kartvelian _*q’ar-/*q’r-_ ‘to rot, to stink’.

6. Proto-Nostratic _*q’aw-_ (~ _*q’əw-_) ‘head, forehead, brow’:

   A. Proto-Afrasian _*k’aw-_ ‘forehead, brow’;
   B. Proto-Kartvelian _*q’ua-_ ‘forehead; handle (of an axe)’;

C. Indo-European: Proto-Germanic **kew-la-z* 'head, top, summit, peak'.

7. Proto-Nostratic **q'el-* 'neck, throat':

   A. Proto-Kartvelian **q'el-* 'neck, throat';
   B. Proto-Indo-European **k'el-*/**k'l̥-* '(n.) neck, throat; (vb.) to swallow'.

8. Proto-Nostratic **q'in-* '(vb.) to freeze, to be or become cold; (n.) cold, frost':

   A. Dravidian: Kolami *kinani*, *kinām* 'cold'; Gondi *kinan*, *kīnd* 'cold', *kinnān* 'wet, cool', *kinnīta* 'cold';
   B. Proto-Kartvelian **q'in-* 'to freeze'.

9. Proto-Nostratic **q'ud-* (~ **q'od-*) 'dwelling, abode, house':

   A. Dravidian: Tamil *kuṭi* 'house, abode, home, family, lineage, town, tenants', *kuṭikai* 'hut made of leaves, temple', *kuṭical* 'hut', *kuṭicai*, *kuṭiñai* 'small hut, cottage', *kuṭimai* 'family, lineage, allegiance (as of subjects to their sovereign), servitude', *kuṭiy-āḷ* 'tenant', *kuṭiyilār* 'tenants', *kuṭil* 'hut, shed, abode', *kuṭaṅkar* 'hut, cottage'; etc.;
   B. Proto-Kartvelian **q'ud-* 'house';
   C. Proto-Finno-Ugrian **kota* 'tent, hut, house'.

Proto-Nostratic *q'ʷ:

1. Proto-Nostratic **q'ʷal-* (~ **q'ʷəl-*) '(vb.) to call (out), to cry (out), to shout; (n.) call, cry, outcry, sound, noise, hubbub, uproar':

   A. Proto-Afrasian **k'ʷal-* 'to call (out), to cry (out), to shout';
   B. Dravidian: Tamil *kulai* 'to bark (as a dog), to talk incoherently', *kulaippu* 'barking, snarling', *kulavai* 'chorus of shrill sounds'; Malayalam *kulākulā* imitative of barking. Kannaḍa *gullu* 'loud noise, hubbub'; Telugu *gollu* 'noise, hubbub, uproar', *kolakola* 'noise, tumult', *golagola* 'a confused noise', *gōla* 'loud noise or outcry', *gulgu* 'to grumble'; Tuḷu *gullu* 'a great noise, shout, uproar';
   C. Indo-European: Greek βληχή (Doric βλᾱχᾱ́) (< **k'ʷl-ā-* < **k'ʷl-eA-* [**k'ʷl-aA-*]) 'a bleating, the wailing of children'; Old High German *klaga* 'cries of pain; complaint, lament, lamentation, grievance' (New High German *Klage*);
   D. Proto-Chukchi-Kamchatkan **quli-* 'to cry or shout'.

2. Proto-Nostratic **q'ʷal-* (~ **q'ʷəl-*) '(vb.) to strike, to hit, to cut, to hurt, to wound, to slay, to kill; (n.) killing, murder, manslaughter, destruction, death':

   A. Proto-Afrasian **k'ʷal-* 'to strike, to hit, to cut, to kill, to slaughter';
   B. Dravidian: Tamil *kol* (*kolv-*, *ko̲n̲r̲-*) 'to kill, to murder, to destroy, to ruin, to fell, to reap, to afflict, to tease', *kolai* 'killing, murder, vexation, teasing'; Malayalam

kolluka ‘to kill, to murder’, *kollika* ‘to make to kill’, *kolli* ‘killing’, *kula* ‘killing, murder’; etc.;
C. Proto-Kartvelian **q’wal-* ‘to slay, to kill’;
D. Proto-Indo-European **k’ʷel-/*k’ʷol-/*k’ʷl̥-* ‘to strike, to hit, to cut, to hurt, to wound, to slay, to kill’;
E. Proto-Uralic **kola-* ‘to die’.

3. Proto-Nostratic **q’ʷal-* (~ **q’ʷəl-*) or **k’ʷal-* (~ **k’ʷəl-*) ‘(vb.) to throw, to hurl; (n.) sling, club; throwing, hurling’ (probably identical to **q’ʷal-* ‘to strike, to hit, to cut, to hurt, to wound, to slay, to kill’):

A. Proto-Afrasian **k’ʷal-* ‘to throw, to hurl’;
B. Proto-Indo-European **k’ʷel-/*k’ʷol-/*k’ʷl̥-* ‘to throw, to hurl’.

4. Proto-Nostratic **q’ʷar-* (~ **q’ʷər-*) ‘edge, point, tip, peak’:

A. Proto-Afrasian **k’ʷar-* ‘highest point, top, peak, summit, hill, mountain, horn’;
B. Dravidian: Tamil *kuṟam* ‘Kuṟava tribe’, *kuṟiñci* ‘hilly tract’, *kuṟicci* ‘village in the hilly tract, village’, *kuṟavāṇar* ‘the Kuṟava tribe of the mountain’; Malayalam *kuṟavan* ‘wandering tribe of basket-makers, snake-catchers, and gypsies’, *kuṟumpan* ‘shepherd, caste of mountaineers in Wayanāḍu’, *kuṟicci* ‘hill country’, *kuṟicciyan* ‘a hill tribe’; Toda *kurb* ‘man of Kurumba tribe living in the Nilgiri jungles’, *kuṟumba* ‘a caste of mountaineers’; Telugu *koṟava* name of a tribe of mountaineers;
C. Proto-Kartvelian **q’ur-* ‘edge’;
D. Proto-Indo-European **k’ʷer-/*k’ʷor-/*k’ʷr̥-* ‘hill, mountain, peak’;
E. (?) Altaic: Mongolian *qorγa* ‘fort, fortress; shelter, enclosure’; Old Turkic *qurγan* ‘castle, fortress’.

5. Proto-Nostratic **q’ʷar-* (~ **q’ʷər-*) or **q’ʷur-* (~ **q’ʷor-*) ‘(vb.) to call out, to cry out; (n.) call, cry, shout’:

A. Afrasian: Semitic: Arabic *ḳaraẓa* ‘to praise, to commend, to laud, to extol, to acclaim’;
B. Dravidian: Tamil *kūṟu* (*kūṟi-*) ‘to speak, to assert, to cry out the price, to cry aloud, to proclaim’, *kūṟṟam* ‘word’, *kūṟṟu* ‘proclamation, utterance, word’; Malayalam *kūṟuka* ‘to speak, to proclaim’, *kūṟṟu* ‘call, cry of men, noise’, *kūṟṟam* ‘cry (as for help)’; Kannaḍa *gūrṇisu*, *gūrmisu* ‘to murmur or roar (as water of a river or the sea), to sound (as a trumpet), to roar or bellow, to cry aloud’; Tuḷu *gūruni* ‘to hoot’; Telugu *ghūrṇillu* ‘to sound, to resound’ (*gh-* is from Sanskrit *ghūrṇ-* ‘to move to and fro’ [> Telugu *ghūrṇillu* ‘to whirl, to turn around’]);
C. Proto-Kartvelian **q’ur-* ‘to howl (of wolves, dogs)’;
D. Proto-Indo-European **k’ʷer-/*k’ʷor-/*k’ʷr̥-* ‘to make a sound, to call, to call out, to praise’.

6. Proto-Nostratic **q'ʷarʸ-* (~ **q'ʷərʸ-*) or **q'ʷurʸ-* (~ **q'ʷorʸ-*) '(vb.) to hear; (n.) ear':

   A. Dravidian: Tamil *kuṟai* 'earring, ear'; Malayalam *kur̤a* 'earring, ear'; Kannaḍa *kɔḍaṅgè* 'earring', *kuḍka*, *kuḍki* 'female's ear ornament'; Kolami *kuḍka* 'earring in the upper ear'; Gondi *kuṛka* 'earring';
   B. Proto-Kartvelian **q'ur-* 'ear', **q'ur-u-* 'deaf, dumb';
   C. (?) Indo-European: Lithuanian *girdžiù*, *girdė́ti* 'to hear', *girdà* 'hearing'; Latvian *dzìrdu*, *dzìrdêt* 'to hear'.

7. Proto-Nostratic **q'ʷatʸʰ-* (~ **q'ʷətʸʰ-*) '(vb.) to say, to speak, to call; (n.) call, invocation, invitation, summons':

   A. Proto-Indo-European **k'ʷetʰ-/*k'ʷotʰ-* 'to say, to speak, to call';
   B. Uralic: Proto-Finno-Ugrian **kutʸз-* 'to call, to summon';
   C. (?) Chukchi-Kamchatkan: Proto-Chukotian **qəððiðæ-* 'to pester, to annoy, to bother, to bore' (assuming semantic development as in Ostyak / Xanty [Southern] *hut'-*, [Nizyam] *hŭś-* 'to call, to entice, to seduce, to incite; to tease, to provoke').

   Note: Also found in Northwest Caucasian: cf. Proto-Circassian **q'ᵒatʰa* 'to tell, to report; to announce, to make known' > Bžedux *ˀᵒātʰa*, Kabardian *ˀᵒāta* 'to tell, to report; to announce, to make known'.

8. Proto-Nostratic **q'ʷur-* (~ **q'ʷor-*) '(vb.) to swallow; (n.) neck, throat':

   A. Afrasian: Semitic: Śḥeri / Jibbāli *ḳɛrd* 'throat'; Ḥarsūsi *ḳard* 'throat'; Mehri *ḳard* 'voice, throat';
   B. Proto-Kartvelian (**q'worq'-* >) **q'orq'-* 'throat, gullet';
   C. Proto-Indo-European **k'ʷor-/*k'ʷr̥-* (secondary *e*-grade form: **k'ʷer-*) '(vb.) to swallow; (n.) neck, throat';
   D. Proto-Finno-Ugrian **k[ü]rkз* 'neck, throat' > Finnish *kurkku* 'throat'; Mordvin (Erza) *kirga*, *kiŕga*, *korga* 'neck'. Note: Finnish *kurkku* is usually considered to be either a Scandinavian loan-word or to have been influenced by Scandinavian.

Corroborating Evidence

In our joint monograph, *The Nostratic Macrofamily: A Study in Distant Linguistic Relationship*, John C. Kerns tried to show that the most likely homeland of the Nostratic parent language was located "in or near the Fertile Crescent just south of the Caucasus". In his 1998 book, *The Nostratic Hypothesis and Linguistic Paleontology*, Dolgopolsky places the homeland in the same general area. In my forthcoming book, *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology, and Vocabulary*, I propose that "[t]he unified Nostratic parent language may be dated to between 15,000 to 12,000 BCE, that is, at the end of the last Ice Age — it was located in the Fertile Crescent just south of the Caucasus..." As can be seen, Kerns, Dolgopolsky, and I are essentially in agreement about the location of the homeland of the speakers of the Nostratic parent language. If this scenario is correct, we would expect to find evidence of contact between Nostratic and non-Nostratic neighboring languages. A good place to look for such evidence would be the Northwest and Northeast Caucasian languages. Not only are languages of these families still spoken, there are good reasons to believe that, in ancient times, they covered a considerably wider geographic area than they do at present. For example, the Hurrian language, along with the closely-related Urartian, which, according to Diakonoff—Starostin (1996), may have belonged to the Northeast Caucasian language family, was located in "the northeastern Zagros-Taurus corner of the 'hilly flanks' of Mesopotamia." Likewise, Hattic, which was located in central Anatolia, has been claimed by some to be an ancient Northwest Caucasian language. We may note in passing that, according to Nikolayev—Starostin (1994), the Northwest Caucasian (Abkhaz; West [Adyghe: Bžedux / Bžedukh, Temirgoy, Šapsug] and East Circassian [Kabardian]; and Ubyx / Ubykh) and Northeast Caucasian (North Central Caucasian [Nakh] and Northeast Caucasian proper [Avar-Andi-Dido; Lak-Dargwa; and Lezgian]) language families are related. Together, they form a larger North Caucasian family.

An examination of the vocabularies of the Northwest Caucasian languages, in particular, shows that there is indeed evidence of very ancient contact between this family and Nostratic languages. I have listed that evidence above as it pertains to the sampling of Nostratic material given in this Appendix — there is much more. The evidence given here for the forms with initial glottalics is especially significant in that it independently corroborates the Proto-Nostratic reconstructions I have proposed, not only the glottalics but the postvelars and labialized velars and postvelars as well.

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MOTHER TONGUE
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Comparing Consonant Series: lessons from Southeast Asian languages for Nostratic.

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As is well known in linguistic circles, I was first attracted to historical-comparative reconstruction a decade and a half ago by the tantalizing prospects raised by Nostratic theory for a contribution to our understanding of Ice-Age man and his later dispersals through the Old World. Subsequently I did not take up the challenge of working within a Nostratic framework, embarking instead on comparative Mon-Khmer, which has now become my (more or less) full-time occupation. None-the-less I have continued to follow Nostratic research, and from time to time waded in with minor contributions, mostly in the form of reviews and review articles (e.g. Sidwell 1996, 1998). One of the issues that has particularly concerned me over the years has been the simmering feud, now two decades old, between the so-called "Muscovite" and "Glottalic" formulations for the Nostratic stop correspondences, which is the theme of this is issue of Mother Tongue. In this context I was pleased to be invited to make a contribution on the topic, and take this opportunity to lay out my current views in this short paper.

So far as I can tell, the discussion this far has focused on two basic arguments. One is typological, and deals with the relative plausibility and likelihood of phonological systems and developments reconstructed by each camp. The other type of argument is more concrete and concerns the etymological specifics of the evidence cited in support of each model. The latter approach can be paraphrased along the lines of "my comparisons are right, your comparisons are wrong", and given the fact of my professional specialisation in a different language family I cannot pretend to be competent to deal in such arguments concerning Nostratic. This said, in the present paper I will deal only with matters of typology, of phological systems and phonological changes, citing data from Mon-Khmer languages, on the assumption that language typology is equally informed by all language families, with no possibility of special pleading available, lest we undermine the fundamental uniformitarianism of the scientific method.

I begin with a quote from Bomhard (2007:19) which sets out nicely the core of the issue being discussed here:

> The mistake that Illič-Svityč and Dolgopolsky made was in trying to equate the glottalized stops of Proto-Kartvelian and Proto-Afrasian with the traditional plain voiceless stops of Proto-Indo-European. This reconstruction would make the glottalized stops the least marked members of the Proto-Nostratic stop system. Illič-Svityč's and Dolgopolsky's reconstruction is thus in contradiction to typological evidence, according to which glottalized stops are uniformly the most highly marked members of a hierarchy. On the basis of these examples, they assumes that, whenever there is a voiceless stop in the Proto-Indo-European examples they cite, a glottalic is to be reconstructed for Proto-Nostratic, even when there are no glottalics in the corresponding Kartvelian and Afrasian forms! This

> means that the Proto-Nostratic glottalics have the same frequency distribution as the Proto-Indo-European plain voiceless stops [....] Clearly, this cannot be correct.

These are strong words - the specific conclusion is unambiguously catagorical. Can we ever make such firm claims on what are essentially typological grounds? As I understand it, typology may establish the distribution or likelihood of various linguistic possibilities, but it does not rule out specific possibilities as such - that kind of categorical statement belongs rather to such disciplines as articulatory phonetics or cognitive science. To take the discussion into more concrete territory let us take a typological journey to Southeast Asia.

Focussing on the matter of oral plosive correspondences we proceed to consider some facts pertaining to the reconstruction of proto-Mon-Khmer (PMK) stops, and the necessary typology of phonological changes implied by these. I will demonstrate that in the linguistic context in which I am now accustomed to working, debates over the typological plausibility of whether this consonant series can correspond to that (different) consonant series, and so on, become so trivial as to be almost meaningless. What this shows me is that we must go back to the etymological data again and again, and not *à priori* discard comparisons because they ostensibly violate one or other of our cherished typological assumptions. It is as likely that we yet have to expand our understanding of typology before we invoke it to justify this or that etymological claim.

The following table summarizes the correspondences of monosyllable root initial labial and apical stops for six MK languages, plus the relevant PMK reconstructions. The data are drawn from Shorto's (2006) comparative dictionary, and are uncontroversial among MK specialists.

PMK	Mon	Cambodian	LawaUmphai	Khasi	Muong	Vietnamese
*p	p	ɓ	p	ph	p	ɓ
*b	p	p	ph	p	p	ɓ
*ɓ	ɓ	p	ph	b	ɓ	m
*t	t	ɗ	t	th (~ t)	t	ɗ
*d	t	t	th	t	t	ɗ
*ɗ	ɗ	t	th	d	ɗ	n

These data show rather dramatically that each language has suffered a restructuring of their oral stops to varying extents, resulting in virtually every logically possible outcome that allows at least one contrast of manner to remain in the system (more than 150 MK languages exist, so the examples here are merely illustrative).

The most conservative languages in this set are Mon and Muong. These have merely undergone a merger of their plosives, so that there remains only a contrast of plosive versus implosive. The same typological result was achieved in Cambodian, by via a different route - implosives first merged with the voiced plosives, then voiceless plosives became implosive, and finally voiced plosives devoiced.

Lawa Umphai parallels Cambodian in its first stage, having merged the PMK implosives and voiced plosives. However, in its second stage the voiced stops changed (in an unconditioned shift) to voiceless aspirated plosives. Thus while both Cambodian and Lawa Umphai have eliminated voicing as a contrastive feature of stops, the former now relies upon implosion while the latter relies upon aspiration.

Khasi took a different route, characterized as a Germanic type shift by Haudricourt (1965). With a small number of exceptions we do not understand (perhaps due to dialect mixing?), Khasi preserves the original three way contrast, but has shifted the whole system by a general delaying onset voicing - plain plosives became aspirates, voiced plosives became voiceless, and implosives became voiced plosives.

Vietnamese is perhaps the strangest. It is closely related to Muong, they having separated only in the first Millennium CE. Muong stops preserve the proto-Viet-Muong manners of articulation with Vietnamese taking an innovating path. Vietnamese implosives became nasals, after which voiceless plosives became imploded. This created a language with *implosives as the unmarked series*. This left a major gap in the system (no plosive stops) which was partly filled by the shift of *s > /t/. This accounts for the remarkable fact that Modern Vietnamese lacks a labial plosive.

The Vietnamese state of affairs reminds us of the claim (invoked by Glottalic Theory proponents) that a stop series lacking a labial member may be explained as reflecting an ejective series. The articulatory explanation for this relies on the fact that labial ejectives necessitate the compression of a much larger volume of air in the oral cavity than do places of articulation closer to the larynx. Mirror-wise, velar and uvular implosives are rare since it is more difficult to manipulate an appropriate drop in air pressure in the small supra-laryngeal cavity created by such back oral closures.

In the case of Vietnamese we cannot, and do not need to, invoke the Glottalic explanation to account for the lack of labial plosive. The explanation lies in the patterning of mergers and shifts in manners of articulation, which in this case failed to yield a labial plosive. This is because fortuitously there was no labial frictative available which would have been expected to harden in parallel with the shift of *s > /t/.

Now we come back to Bomhard's claims (above) concerning the implausibility of comparing ejective and plain stops among Nostratic languages, and implications for the relative markedness of the respective stop series within the phonology. It is quite evident that within the Mon-Khmer family, a linguistic genetic unit with is considerably younger and less diverse than Nostratic, that there are unambiguous examples of all types of stops corresponding regularly, often as a result of whole series shifting in their articulation without any specific conditioning. Consequently we observe that, according to each language, we are variously forced to concede that any of aspirate, voiceless plosive, voiced plosive, or implosive series may be the most common or least marked series in the stop phonology. No amount of theoretical considerations or arguments about the way things aught to be can change these facts since they are established in the first place by careful and extensive etymological analyses.

In conclusion then, I am not arguing here for or against any particular model of Nostratic stop correspondences. However, I am arguing that we are risking a big mistake if we reject on systematic-typological grounds, of the sort invoked by Bomhard and other Glottalic Theory adherents, the mass of lexical comparisons proposed by Illich-Svitych, Dolgopolsky and others that involve the disputed stop correspondences. The correspondences of Dolgopolsky are criticized by Bomhard as excessively complex and ad hoc looking, while Bomhard approvingly recommends his own compilation as a model of regularity and phonetic similarity. In such context how could we even begin to accept the possibility of the Mon-Khmer correspondences I have presented here, as it is obvious that we would get nowhere insisting upon comparing implosive with implosive, or aspirate with aspirate, etc.?

An important issue which is not dealt with by either model of Nostratic stop correspondences concerns the very nature and origin of ejective stops. They are highly marked phonetically, and must be counted as less likely to be historically stable over such great time depths as required by Nostratic theory. Why would we believe them to be ancient and persistent (any more so than emphatic stops). Can or should we rule out parallel structural innovations to account for their appearance in Afrasian and Kartvelian languages?

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Comments on A. Bomhard's "The Glottalic Theory"

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Allan Bomhard's paper presents a logical continuation of his research on comparative Nostratic phonology that, as he himself states, goes back to the 70s. To be fair, none of the ideas expressed in this article are particularly new, but they have been somewhat updated in accordance with some of the progress made in Nostratic linguistics, as well as the study of the daughter branches of Nostratic, over the last two decades.

This, by itself, is a major plus, and one thing over which I am definitely in agreement with the author is that the work by pioneers of Nostratic can, and should be subject to modification; nothing in the Nostratic theory as formulated by V. M. Illich-Svitych and A. Dolgopolsky should be considered "sacred", especially in the light of the already mentioned progress that has been achieved in the reconstruction of the daughter branches of Nostratic over the past half century. That said, it is hardly reasonable to introduce any kind of modifications to sound, stable systems unless demand for such modifications becomes truly overwhelming: "if it ain't broke, don't fix it".

One such serious modification is proposed here by Bomhard. While the majority of the article focuses on the «glottalic theory» and its theoretical implications for macro-comparison, in practical terms the main claim here is that a significant part of the phonetic correspondences between daughter branches of Nostratic, established by Illich-Svitych and followed by Dolgopolsky et al., has been established incorrectly, and needs to be "reestablished" on the basis of typological evidence (distributional features of glottalic consonants) and a phonetic reinterpretation of Proto-Indo-European consonatism.

I feel somewhat uneasy about it, in that Allan's "reshuffling" of the correspondences gives the impression of putting typological considerations first and actual language data second. In standard comparative linguistics — and we are all working on Nostratic under the assumption that we are not doing anything that does not fit within standard comparative linguistics — such an approach is hardly admissible. Thus, it is one thing to reinterpret the classic Indo-European consonantal reconstruction in the light of the "glottalic theory", but it is an entirely different thing if such a phonetic reinterpretation also brings along a reassessment of the actual correspondences; if someone, based on typological considerations, started to insist that Greek *k* cannot correspond to Gothic *h*, but should correspond to Gothic *k*, and that we must rewrite the actual Indo-European etymologies, such an idea would hardly be taken seriously — simply because it is obvious for every specialist in the field that it is impossible to make an equally good case for "Greek *k* : Gothic *k*" as it is for "Greek *k* : Gothic *h*".

Since comparative Nostratic is, after all, only a logical extension of comparative Indo-European, Uralic, Altaic, etc., the same argument should work for this macrofamily. Thus:

(a) basic correspondences within the stop system should be one of the main points of consensus on Nostratic (in fact, on any comparison);

(b) the system as initially established by Illich-Svitych looks fairly reasonable in that it is backed up by a decent number of examples (note that I am only speaking about the *correspondences*, not Illich-Svitych's phonetic interpretation of the reconstruction);

(c) if a statement is made that a certain share of these correspondences has to be abandoned in favour of different ones, then, in order for that statement to be acceptable, it has to work on at least an *equally* large number of examples of the *same* (or better) quality;

(d) the notion of 'quality' would encompass such factors as phonetical regularity (in other respects than the stops in question), semantic tightness, and being well-represented in the daughter branches.

Therefore, before any theoretical discussion on the issue, the first thing to do is to test those of Allan's "counter-Svitych" etymologies that are relevant to the issue on whether they are really preferable to the original ones. The test will be conducted as follows: I will evaluate Allan's etymologies both on their own and in comparison with *alternative* ones that are based on Illich-Svitych's correspondences (either suggested by Illich-Svitych himself and rejected by Allan, or the ones proposed later by A. Dolgopolsky and/or the Moscow scholars). If the "old" system wins, hands down, in terms of quality and quantity over the "revised" one, I recommend that the latter be rejected; if it does not, Allan's revisions will have to be taken into account.

Allan's material in the Appendices comprises 79 Nostratic etymologies with initial stops that he reconstructs as "glottalic". However, not every single one of them needs to be discussed. First, Allan's revisions only concern those etymologies that involve Afrasian and/or Kartvelian material; etymologies that do not contain lexical material from these families (e. g. **k'uŋ-* 'buttocks, rump, anus', found in Indo-European, Dravidian, and Altaic) do not violate Illich-Svitych's system. These cases will, therefore, not be analyzed in my comment.

I will also not focus on Uralic and Dravidian parts of the etymologies, since, due to mergers of oppositions in these families, they are irrelevant to the case (it should be noted, though, that there is a serious possibility of reconstructing some of the Dravidian items with initial voiced consonants instead of voiceless, which will also influence the correspondences; this problem, however, also lies beyond the scope of my comments). Therefore, etymologies that do not include Altaic or Indo-European material will be omitted.

Third, I will reduce the material even further by focusing only on those etymologies that feature parallels from Kartvelian. There are several reasons for this: not the least of them is space limitation, but mainly it is due to the fact that (a) following S. Starostin, I consider Afrasian a sister-family rather than a daughter-family of Nostratic (and even Allan himself acknowledges that Afrasian was probably the first branch to split off), and (b) the level of our current knowledge about Proto-Afrasian is, unfortunately, still extremely low in comparison with the much better studied "proper Nostratic" branches.

In the end, it all depends on our treatment of Kartvelian data. If Allan's grouping of Kartvelian items with glottalized stops together with Indo-European voiced (a.k.a. "glottalic") and Altaic voiceless stops can be shown to work better than their grouping together with Indo-European voiceless (a.k.a. "voiceless (aspirated)") and Altaic voiceless aspirated stops, his case will hold. If it cannot, the day will probably not be saved with (generally less reliable) Afrasian data either.

Discussion of relevant etymologies

(My data sources are more or less the same as Allan's, as is the transcription; the only difference is that, since I am not necessarily convinced of the correctness of the "glottalic theory", I will be transcribing Indo-European forms in the traditional way, i. e. Allan's voiceless aspirated = my voiceless, glottalized = my voiced, voiced = my voiced aspirated).

1. PN **p'ap'a-* 'old man, old woman' > PK **p'ap-* 'grandfather', PIE **babā* 'old woman'.

An obviously expressive, and therefore not diagnostic, etymology. Besides, in IE cf. also **appa*, **pap(p)a* 'father, Dad', slightly better semantically.

2. PN **t'aħ-* 'to break, split, etc.' > PK **t'ex-* 'to break', PIE **dā-* 'to cleave, split, divide'.

An acceptable etymology, although it should be noted that the basic meaning of the PIE root is quite obviously 'divide', so from the semantic side the match is rather weak.

3. PN **t'al-* 'to lick' > PK **t'lek'-/*t'lik'-*, PA **tālV-* 'to lick'.

An acceptable — and probably true — etymology. It should, however, be noted that the PK form is not diagnostic; first, the cluster **tl-* is not reconstructed for PK at all (so it can be suggested that PK **tl-* > **t'l-*), second, one cannot exclude regressive assimilation with the suffixal glottalic *-k'-*.

4. PN **t'an-* 'to fill, stuff, etc.' > PK **t'en-* 'to fill, stuff, pack', PIE **dn̥s-u-* 'closely packed'.

Considering the scarce representation of the root in PIE (essentially a Greek-Latin isogloss), the parallel is slightly less convincing than Illich-Svitych's comparison with PIE **ten-* 'to pull' (cf. also PA **t`āno* 'to stretch, pull'); note the common Kartvelian semantics 'to pack closely, stuff to the brim' (= 'stretch to the maximum').

5. PN **t'aq'-* 'to cover, protect' > PK **t'q'aw-* 'skin, hide', PIE **deg-/*steg-* 'to cover'.

The PK stem, along with the nominal meaning 'skin', also has the verbal meaning 'to skin', making the semantic parallel with PIE somewhat dubious. Much more satisfying from the semantic side is a direct comparison with PIE **twak-* 'skin, hide', although we would have to suppose a metathesis in PK (on the other hand, it helps understand the *-aw-* "extension" in PK).

6. PN **t'aw-* 'to go, leave, let go' > PK **t'ew-* 'to leave, let go'; PIE **dew(A)-* 'to go, leave'.

Technically acceptable, although the semantics is far-fetched (PK is primarily 'let go', PIE is rather 'to proceed' or 'far, far away'). No alternative etymology for PK.

7. PN **t'uk'-/*t'ok'-* 'to knock, beat, etc.' > PK **t'k'ač-, *t'k'eč-, *t'k'ič-* 'to hit', **t'k'eb-* 'to press', etc.; PIE **tog-* 'to knock', etc.

Not quite clear which PIE root is being referred to; anyway, the parallel does not quite satisfy Allan's own correspondences. The most obvious IE parallel is **teuk-/*tūk-* 'to beat', a perfect match for Illich-Svitych's correspondences.

8. PN **k'ad-* 'to tie, fasten; build, construct' > PK **k'ed-/*k'd-* 'build, construct'; PA **kadu* 'a kind of harness (bridle)'.

Apparently, the Dravidian parallel **kaṭ-* 'to tie, fasten; build' serves here as a 'semantic bridge' between PK and PA (otherwise, the comparison would have been dismissed out of hand), but the distance is still significant. Illich-Svitych compares the PK and PD items with PIE **ket-, *kot-* 'a k. of (wattled) building', which is phonetically plausible (**k'adV* > *kedh-* > *ket-* according to regular PIE phonotactics) and semantically better than the 'bridle' connection.

9. PN **k'ak'-* 'to cackle' > PK **k'ak'a-n-* id.; PIE **gag-* 'to cackle, to chatter'.

Another expressive formation. While PIE **gag-* may indeed belong here, equally plausible would be a connection with PIE **kak(h)-* 'to laugh' — cf. particularly the Dravidian parallel (**kak-* 'to laugh').

10. PN **k'al-* 'stone, rock' > PK **k'l̥de-* 'rock, cliff'; PIE **g(e)l-* 'rock, stone'.

The PIE root (in case it is really Pokorny's **gel-* 1 that the reconstruction refers to) actually means 'swelling' or 'round, globular'; the meaning 'rock, stone' is encountered in a couple scattered derivations at best. The etymology itself (with its Uralic and Dravidian counterparts) is quite strong, but in IE one could also consider Old Indian *śilā́* 'stone', Armenian *sal* 'stone plaque' < PIE **k̂əl-ā* (Pokorny: < **k̂ē(i)-* 'to sharpen, wet', but with certain doubts).

11. PN **k'aph-* 'jaw, jawbone' > PK **(ni-)k'ap-* 'lower jaw, chin'; PIE **gep-/*gop-* 'jaw, mouth'; PA **kēp`a* 'jaw, face'.

A complicated case with many phonetic uncertainties. First, the PK reconstruction should probably be **(ni-)k'ap'-*; the form *ni-k'ap-* is only attested as a variant in Georgian; all the other languages have the variant with two glottalics. Second, IE has both **gep(h)-* (in Iranic) and (more frequently) **geb(h)-*; this may indicate a "belated" application of the phonotactic restrictions (original **gep-* > **gebh-*), but could as well be explained by an irregular dissimilation in Iranic (Allan separates the two, explaining them as reflexes of two different Nostratic roots, but this is very unlikely given their areal distribution within IE). The Svitych scenario would leave unexplained the *k'*- in Kartvelian (**kap'*- instead of **k'ap'*- would be expected); the Bomhard scenario leaves unexplained (let alone unmentioned) the -*p'*-. Both are equally probable.

12. PN **k'ar*- 'to twist, turn, etc.' > PK **k'ar-/*k'r*- 'to bind, tie'; PIE **ger-/*gor*- 'to twist, turn, bend'; PA **kera* 'to bind, wind around'.

Very broad semantics, meaning that it is not difficult to find "Svitych model" parallels in IE; cf., e. g., **k̑er*- 'rope; to weave' (a Greek-Armenian isogloss) or **(s)ker*- 'to turn, bend', etc.

13. PN **k'ar*- 'protuberance, lump, breast' > PK **m-k'erd*- 'breast, chest'; PIE **ger-/*gor-/*gr̥*- 'protuberance, lump, breast'.

A match between the PK word for 'breast' and a PIE root one of the extensions of which also means 'breast' (Slavic **grǫdъ*) may seem convincing. However, neither Pokorny nor Vasmer are ready to really group the Slavic forms together with Lithuanian *grùbas*, etc., like Allan does; phonetically they rather belong together with Latin *grandis* 'big' and Greek *brenthos* 'pride' < PIE **g^{w}rendh*- 'lofty; high place'. With the Slavic forms gone, the others are hardly a better match for PK than the traditional Svitych etymology (= PIE **k̑erd*- 'heart', with a regular development **k̑erdh*- > **k̑erd*- due to phonotactic restrictions).

14. PN **k'aw*- 'to bend, twist, curve' > PK **k'w-er*- 'round object'; PIE **gew-/*gow*- 'bent, curved; round object'.

The PK form is compared by Illich-Svitych with IE **k^{w}el*- 'wheel; round' < PN *k'oĺV*; Bomhard's connection is no worse semantically, but the Svitych etymology helps avoid extra segmentation for Kartvelian. (On an indirectly related note, the Dravidian form should be reconstructed as **gav*- and is probably connected to PIE **geup*- 'cave', grouped by Pokorny together with **gew*-).

15. PN **k'aw*- 'to take, seize, grasp' > PK **k'aw*- 'to take'; PIE **gow(H)*- 'to take, seize'.

Generally acceptable (the PK form has no other etymology), although the distribution of the forms within PIE is rather weak.

16. PN **k'ep'*- 'to cut, chop, chew' > PK **k'ep'*- 'to cut, mince'; PA **kēpu* 'to chew'.

Acceptable, but weak semantics. Illich-Svitych compares the PA root with PIE **ĝyew*- 'to chew', which is perfectly fine if the inlaut consonant in PA is -*b*- (EDAL's reconstruction **kēpu* is based on the preservation of inlaut labial stop in Mongolian **kebi*, but an alternative variant **kējbu* is not excluded), and, predictably, leaves out the Kartvelian parallel. The Altaic-IE comparison is thus weaker phonetically, but much stronger on the semantic side.

17. PN **k'er*- 'to gather, collect' > PK **k'er-b-/*k'reb*- 'gather, collect'; PIE **ger-/*gor*- 'gather'.

Acceptable, but cf. Illich-Svitych's equally plausible parallel in PIE **kerp*- 'to gather (fruit)'. Pokorny's derivation of the root from **(s)ker*- 'to cut' is arbitrary; if the match with

Kartvelian is correct, the labial consonant must have formed part of the original Nostratic root.

18. PN **k'ir-* 'to cut' > PK **k'r-eč'k'-*, etc. 'to cut'; PIE **ger(bh)-* 'to cut'; PA **kiro* 'to cut, mince'.

In PIE cf. alternatively **kert-* 'cut', in PA — **k`ire-gV* 'cutting tool'. Note that PIE **ger-* 'cut' does not really exist, and the semantics of **gerbh-* is quite specific ('make scratches, marks, incisions').

19. PN **k'um-* 'to sigh, weep, lament' > PK **k'um-in-* 'to moan, grumble'; PIE **gom-/*gm-* 'to sigh, weep'.

Possible, although the root belongs to the expressive part of the lexicon; besides, it is very poorly represented in PIE (Latin, Celtic and Armenian; not found in Pokorny's dictionary).

20. PN **k'um-* 'to press together' > PK **k'um-* id.; PIE **gem/*gom-/*gm-* 'id.; to seize, grasp'.

Quite acceptable — but cf., alternatively, PIE **kem-* 'to press, squeeze', nominal derivative **komo-* 'lump'.

21. PN **k'un-* 'to bend; to tie together' > PK **k'on-* 'to tie together'; PA **kunu* 'to fold, twist'.

Alternately, cf. PIE **kenk-* 'to tie, bind'; PA **k`ū̀ńi* 'knot, to tie knots'.

22. PN **k'ut'-* 'short, small' > PK **k'ut'-* 'little, small'; PA **ki̯ūta* 'insufficiency, debt'.

Average semantics, but on the whole, acceptable.

23. PN **q'ab-* 'jaw' > PK **q'ab-* id.; PIE **gebh-/*gobh-* id.

See 11 above. There is little reason to find two separate PIE roots here.

24. PN **q'alY-* 'sexual organs' > PK **q'al-* 'penis'; PIE **gel-t-* 'vulva, womb'.

Questionable semantics (male and female genitalia do not mix very frequently) and very poor distribution in PIE (only Indic and Germanic). Several possibilities of alternate etymologies in IE — cf., e. g., **k̂ol-/*k̂el-* 'thorn, awn' (with better distribution and actually *better* semantics) or **k̂al-* 'pointed stick' (both forms are glossed together in Pokorny but may represent different roots).

25. PN **q'aw-* 'head, forehead, brow' > PK **q'ua-* 'forehead'; PIE > Germanic **kew-la-z* 'head, top, summit, peak'.

The PK word is sometimes thought of as borrowed from PIE **kowā* 'back of axe'; even if it is not, the IE distribution (only Germanic, with a proposed suffixal extension) is extremely poor.

26. PN **q'el-* 'neck, throat' > PK **q'el-* id.; PIE **gel-/*gl̥-* 'neck, throat'.

A perfect alternative can be found in PIE **kol-/*kel-* 'neck'.

27. PN **q'*^{w}*al-* 'to strike, hit, kill' > PK **q'wal-* 'to slay, kill'; PIE **g*^{w}*el-/*g*^{w}*ol-* 'to strike, hit, kill'.

Perfectly acceptable both phonetically and semantically. It should, however, be noted that both PK and PIE show a whole bunch of verbal roots with a KL- structure and 'destructive' semantics (PK: _*gal-_ 'tear, break', _*ḳal-_ 'kill', _*qal-_ 'beat, break'; PIE: _*kel-/*kol-_ 'hit, cut down'), so it is not entirely clear what should be corresponding to what.

28. PN _*q'^{w}ar-_ 'edge, point, peak' > PK _*q'ur-_ 'edge'; PIE _*g^{w}er-/*g^{w}or-_ 'hill, mountain'.

Extremely weak semantics and poor representation in Kartvelian. An alternative etymology, however, is only possible if we accept S. Starostin's idea of the fourth row of Nostratic correspondences (S. A. Starostin, _Ob odnom novom tipe sootvetstviy shumnykh smychnykh v nostraticheskikh yazykakh [On one new type of stop correspondences in Nostratic languages]_, in: S. A. Starostin. Trudy po yazykoznaniyu [Works on linguistics], Moscow, Yazyki slavyanskikh kul'tur, 2007, pp. 803-805) — in this case we can compare the PIE root with PK _*gora_ 'hillock'.

29. PN _*q'^{w}ar-_ 'call out, shout' > PK _*q'ur-_ 'to howl'; PIE _*g^{w}er-/*g^{w}or-_ 'to make a sound, call'.

Onomatopoeic again, and therefore not very reliable. IE forms with an initial voiceless can also be found (e. g. _*ker-/*kor-_ 'a k. of sound (shrieking)').

30. PN _*q'^{w}ur-_ 'to swallow' > PK _*q'orq'-_ 'throat'; PIE _*g^{w}or-/*g^{w}er-_ 'to swallow; neck, throat'.

PK also has _*qarq-/*qorq-_ 'throat, pharynx'; in addition, the relations between PK _*q'orq'-_ and North Caucasian _*q'aq'ari/*q'araq'i_ are unclear (may be a borrowing from NC or influenced by the neighbouring forms). In any case, the word also belongs to the expressive layer.

The results of our survey of Allan's 30 comparisons of Kartvelian material with diagnostic Indo-European or Altaic forms are as follows:

In 8 cases, alternative etymologies following the "unrevised" correspondences can be found that are unquestionably better from the semantic point of view (1, 5, 8, 10, 13, 16, 18, 24).

In 11 cases, similar alternatives can be found where semantic distance between the compared forms is more or less the same (4, 7, 9, 12, 14, 17, 20, 21, 26, 29, 30).

In 3 cases, no alternatives are found, but the comparison raises semantic doubts (2, 6, 22).

In 3 cases, no alternatives are found, but the correspondences are ambivalent due to irregularities (possibly dialectal) in daughter branches or between branches (3, 11, 23).

In 3 cases, phonetics and semantics are acceptable, but the word is very poorly represented in diagnostic branches (15, 19, 25).

In 1 case, an alternative can be found if Starostin's "fourth row" is admitted (28). Without going into too many details, the argumentation presented in the mentioned article on the material of 30 new etymologies for the additional correspondence "IE voiced : Altaic voiceless aspirated : Kartvelian voiced", overall, seems more solid than the one shown here (better semantics, less expressive lexicon, etc.).

In 1 case, no serious objections can be raised except for the word belonging to a "word family" with several old roots similar phonetically and semantically, so contamination is not excluded (27).

Additional concerns: quite a few items belong to the 'expressive' layer (not very good material for proving important correspondences); very few items from the 'basic lexicon', in particular, the proposed subset of correspondences covers only one match between Indo-European and Kartvelian on the 100-wordlist (27, and even that with doubts).

Let us now see how fares the more "traditional" version of the comparison by supplementing the 20 "alternate" etymologies for Allan's examples (in many cases this simply means going back to the original propositions of Illich-Svitych and Dolgopolsky) with other material, either old one that he is not mentioning or new one that he may not be aware of.

First, in several cases the counterevidence is simply separated by Allan into "variant" Proto-Nostratic stems. This covers the following evidence:

(a) PN _*t'ab-_ 'to be or become warm' > PK _*t'eb-/*t'b-;_ cf. Altaic _*t`ep`V/*t`ebV_ 'to burn; be warm', PIE _*tep-_ 'to warm; warmth' (in IE — the same development as in (8)). There is no need to set up a separate PN _*theph-_ to account for the difference (PK does not have a second variant like _*tep-_);

(b) PN _*k'el-_ 'female-in-law' > PIE _*g(a)lowV_ 'husband's sister'; PA _*kele_ 'daughter-in-law'. Why this has to be separated from PK _*kal-_ 'woman' is unclear; Kartvelian has no root like _*k'al-/*k'l-_ that could be compared to the PIE and PA forms;

(c) PK _*k'ud-_ 'tail' (31) should reasonably belong not with the various forms for sexual organs, but with PA _*k'i̯údo(rgV)_ 'tail' and possibly IE _*kaud-_ id. (if the Latin form _caudā_ is an archaism — which is possible, given that almost all the subbranches of IE have their own word for this body part).

In addition, the following Kartvelian words with glottalics can be compared to PIE parallels with voiceless stops and PA parallels with voiceless aspirates:

(d) PK _*p'ir-_ 'edge'; cf. PA _*p`eri_ id. (+ IE _*per-_ 'before'?);

(e) PK _*p'u-_ 'to cut, hack'; cf. PIE _*pewə-_ 'to beat, hew';

(f) PK _*p'ir-_ 'to like, wish'; cf. PA _*p`ero_ 'to wish, desire' (+ IE _*per-_ 'to attempt'?);

(g) PK _*p'erp'er-_ 'butterfly'; cf. PA _*p`ep`a_ id., PIE _*pāpel-_ id. (an obviously expressive form, but a very distinct one);

(h) PK _*t'iź-_ 'louse'; cf. PA _*t`ijV_ id.;

(i) PK _*t'p-_ 'to spit'; cf. PA _*t`up`i_ id.;

(j) PK _*t'rp-_ 'to enjoy, like'; cf. PIE _*terp-_ 'to prosper';

(k) PK _*k'ać-_ 'to cut, chop'; cf. PIE _*k'es-_ 'to cut', PA _*k'asi_ id.;

(l) PK _*k'ep-_ 'nape, skull'; cf. PIE _*kaput-_ 'head';

(m) PK _*k'wes-/k'us-_ 'to moan, sigh'; cf. PIE _*k̂wes-_ 'to pant, sob, sigh', etc.

These are but the best of about 50-60 examples of this type that I adduce here — ones that are not connected with any additional phonological problems and have perfect or near-perfect semantics. Note that even the expressive lexemes are of a better quality: there is little reason to doubt the validity of words like 'spit' or 'butterfly', since they do not usually belong in a large 'class' or 'family' of similar onomatopoeic formations.

In the light of this, I see no reason whatsoever to substitute the original correspondence "PIE voiceless : PA voiceless aspirated : PK glottalized", as proposed by the "fathers" of Nostratic, for "PIE voiced (a.k.a. glottalized) : PA voiceless : PK glottalized" as espoused by Allan. The exclusive part of his "massive counterevidence", upon close examination, boils down to about 10 etymologies of varying quality, whereas the exclusive part of the "traditional" evidence includes a much bigger number of direct semantic matches, and "killing off" that evidence in an illusionary goal of getting more rigorous proof for Nostratic would simply mean cutting the branch upon which we happen to be sitting.

What, then, of Allan's typological argumentation? I do not wish to go into major details here, since etymological evidence should always dominate over typological considerations, but a few points can be made. First, Allan says that it cannot be correct if PN glottalics have the same frequency distribution as the Proto-Indo-European plain voiceless stops, since "glottalized consonants are uniformly the most highly marked members of a hierarchy". But if we look at Kartvelian data more closely, it turns out that in this case at least this statement can be easily challenged. Consider the following distribution of roots with initial stops in the Kartvelian database that contains 1310 entries taken from Klimov's and Faehnrich's dictionaries:

Phoneme	#	Phoneme	#	Phoneme	#	Phoneme	#
*b-	83	*g-	50	*d-	44	*ʒ- + *ʒ́-	50
*p-	59	*k-	42	*t-	36	*c- + *ć-	63
*ṗ-	19	*ḳ-	106	*ṭ-	53	*c̣- + *ć̣-	112

It turns out that the "most highly marked" principle only works in the case of the labial stop — in *every* other instance glottalized consonants are obviously the *least* highly marked ones in the system! Granted, we can avoid this confusing situation by phonologically reinterpreting the Kartvelian opposition "voiced — glottalized — voiceless" as "voiced — voiceless unaspirated — voiceless aspirated" (since all the voiceless consonants have an additional feature of aspiration); then the "voiceless unaspirated" consonants will be the least marked, which will suit us just fine. In fact, it is not excluded that this was just the way this opposition initially looked like in Kartvelian, with the voiceless unaspirated consonants adopting glottalic articulation later (quite possibly — under heavy North Caucasian influence; it is, after all, hardly a coincidence that, out of all the daughter branches of Nostratic, if we exclude Afrasian, only Kartvelian utilizes glottalized consonants *and* is adjacent to a nearby linguistic neighbour that makes heavy use of the same feature).

But if Kartvelian glottalic consonants weren't originally glottalic, *there is no need either to necessarily interpret Illich-Svitych/Dolgopolsky's reconstructed glottalized consonants as glottalized.* The correspondence "PIE **k* : PA **k*\` : PK **ḳ*" may, for instance, reflect just a lax PN **k*, whereas "PIE **g* : PA **k* : PK **k*" may reflect a tense PN **k̄* (**kk*), and "PIE **gh* : PA **g* : PK **g*" can be interpreted as a PN voiced **g*. We may tentatively offer other scenarios as well (it should be noted that no exact consensus has been reached on the interpretation of the triple opposition in PA either), including ones motivated by adopting the glottalic theory for IE — although, since we have just shown that trying to match the "new" IE "glottalics" with PK glottalized stops does not work too well both from the etymological and the distributional point of view, it is not clear what implications this adoption should have for the Nostratic theory.

In short, I am not bothered at all by any reinterpretation of Nostratic *phonetics* that can be suggested based on Allan's typological evidence. I think that the discussion laid out in the main body of the article is sound from a theoretical point of view. It's only when the actual data starts getting analyzed that the flaws of this approach become clearly visible.

To be fair, I should note that, since only Kartvelian material was taken here as diagnostic, it is not excluded that Allan's case might still hold, at least partially, for Afrasian, where glottalic (emphatic) stops *are* indeed more highly marked. But before pronouncing any final word on this side of the matter, it would be commendable to at least have a highly reliable source on comparative Afrasian, since none of the existing dictionaries so far (Diakonoff et al., Ehret, Orel-Stolbova) can be called sufficiently satisfactory for us to be able to use them without thinking twice for further external comparison.

MOTHER TONGUE
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Comments on the Comments

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Introduction

There is a profound difference between dismissal and dialog. They reflect two different states of mind and two different approaches to dealing with ideas with which one disagrees. Typically, dismissal reflects arrogance founded on bias. An example here would be the British parliamentarian Edmund Burke's unwillingness to discuss the work of the French *philosophes*, whose secular and rationalist ideas he regarded as such an affront to his own deep-rooted religious views that he did not even deem them worthy of consideration. That is to say that his preconceived ideas, his prejudices, prevented him from even considering alternative points of view, no matter how valid or appealing they might otherwise have been. Dialog, on the other hand, suggests a willingness to engage those with whom one disagrees. It also suggests a willingness not only to consider alternative points of view on their own merits but also to embrace those points of view when it can be shown that they are more rational, more in line with the facts, more beneficial, or in some other way superior to one's own cherished ideas. It further implies an obligation to take the time to present, in a calm, detached, and rational manner, counterarguments and/or counterevidence when alternative points of view are so out of line with the facts that they demand a rebuttal. An example here would be the responsibility on the part of the scientific community to refute the claims of those who advocate "creation science," "intelligent design," and other such preposterous theories in an attempt to undermine the despised "Theory of Evolution."

Those who fall into the first category, that is, "dismissal," use various well-known techniques to avoid having to engage with others who are presenting alternative points of view. The first technique is simply to treat the alternative views as though they do not exist, to ignore them completely, perhaps hoping that they will go away. This may be called "avoidance." Another technique is to engage in *ad hominem* attacks. Knowing or suspecting that the alternative points of view might have a degree of validity, one attacks the person presenting those views instead of dealing with the views themselves. The thinking here being that, if the character or integrity or education or social status or physical appearance or some other arbitrary quality of the person presenting the views can be sufficiently demeaned, no one will pay any attention to what he or she is saying. Another technique is known as the "appeal to authority." We see this technique used, for instance, by those who support "creation science" or "intelligent design." The argument runs along the lines that: "The Theory of Evolution cannot possibly be true because it is in disagreement with what is contained in Holy Writ, and only Holy Writ matters." The flaw in this technique is blatantly obvious — if the "authority" turns out to be wrong, then everything that follows is wrong. The next technique is to distort, either purposely and maliciously or unwittingly through lack of understanding, the alternative view and then to attack the distortion as though it were the actual alternative view. A clever distortion can often be so subtle and so convincingly presented that it can fool all but the most astute and informed reader or listener. Politicians seem particularly adept at this technique. Another technique is simply to bully the opposition into submission. Tyrants and dictators have used this technique for ages. This technique can be subdivided into different tactics that involve varying degrees of coercion. And so on, and so forth. We should recognize all of these techniques for what they are and not be misled by them.

There is a way to tell, by the nature of one's initial reaction to an alternative point of view, whether one is being rational or irrational, open-minded or biased. If one reacts with anger, this may be a warning sign, as so clearly articulated by Bertrand Russell (1976:116):

> If an opinion contrary to your own makes you angry, that is a sign that you are subconsciously aware of having no good reason for thinking the way you do. If some one maintains that two and two are five, or that Iceland is on the equator, you feel pity rather than anger, unless you know so little about arithmetic or geography that his opinion shakes your own contrary conviction. The most savage controversies are those about matters as to which there is no good evidence either way. Persecution is used in theology, not in arithmetic, because in arithmetic there is knowledge, but in theology there is only opinion. So whenever you find yourself getting angry about a difference of opinion, be on your guard; you will probably find, on examination, that your belief is going beyond what the evidence warrants.

Claims and Assumptions

In my paper "The Glottalic Theory of Proto-Indo-European Consonantism and Its Implications for Nostratic Sound Correspondences," I make three fundamental claims:

1. The traditional reconstruction of the Proto-Indo-European consonant system is flawed and is to be reinterpreted along the lines proposed by Thomas V. Gamkrelidze and Vjačeslav V. Ivanov and, independently, by Paul J. Hopper, as follows (the reconstruction of the Proto-Indo-European stop system proposed by Winfred P. Lehmann [1952:99] is given for comparison):

Lehmann				Gamkrelidze(—Ivanov)		
b	b^{h}	p	=	p’	bh/b	ph/p
d	d^{h}	t	=	t’	dh/d	th/t
g	g^{h}	k	=	k’	gh/g	kh/k
g^{w}	g^{wh}	k^{w}	=	k’	gh/g	kh/k

2. The frequency distribution of Proto-Nostratic stops in the reconstruction proposed by Vladislav M. Illič-Svityč and Aharon Dolgopolsky is in contradiction to typological predictions, and is, consequently, highly suspect.

3. Taking into consideration (1) the radical reinterpretation of the Proto-Indo-European consonant system proposed by Gamkrelidze, Ivanov, and Hopper, as well as (2) the problems in the frequency distribution of stops in the reconstruction of the Proto-Nostratic phonological system proposed by Illič-Svityč and Dolgopolsky, a different set of Nostratic sound correspondences is warranted.

Each of these claims must be evaluated independently. The reasons that each claim must be evaluated independently are as follows: Even if claim 1 proves to be untenable, it does not invalidate claim 2. Likewise, even if claim 2 proves to be untenable, it does not invalidate claim 1. Claim 3, on the other hand, is dependent upon claim 2 but not claim 1. That is to say, claim 3 is not dependent upon any particular reconstruction of the Proto-Indo-European consonant system, though, it goes without saying, if claim 1 is valid, it reinforces the likelihood that the revised set of Nostratic sound correspondences I have proposed is correct. Inasmuch as claim 3 is dependent on claim 2, however, if claim 2 is invalid, then claim 3 is unnecessary. Moreover, even if claim 2 is valid and a different set of Nostratic sound correspondences is warranted, it does not necessary follow that the alternative correspondences that I have proposed are valid.

Let us now turn to the individual papers submitted as comments on my paper. In so doing, whether the claims and assumptions made in this section have or have not been addressed by the authors as well as how they have been addressed will be discussed where appropriate.

Paul Sidwell's Comments

The main thrust of Sidwell's comments appears to be to caution against the indiscriminate use of typology. The point is well taken. The judicious use of typology should only be used as an adjunct to traditional methodologies such as the Comparative Method and Internal Reconstruction, never as a substitute for those methodologies. I might add to Sidwell's warning that language comparison should also be data-oriented as opposed to of theory-oriented. That is to say, the data should take precedence and should never be manipulated, distorted, ignored, or falsified to fit a set of preconceived ideas.

George Starostin's Comments

First, I need to acknowledge that Starostin found a legitimate error in the original version of my paper in the section entitled "Critique of Moscovite Views." For this, I am extremely grateful, and I have subsequently corrected that error. The original paragraph was a follows (the problematic wording is in italics) (I now prefer "Afrasian" in place of "Afroasiatic" or "Hamito-Semitic"):

> The mistake that Illič-Svityč and Dolgopolsky made was in trying to equate the glottalized stops of Proto-Kartvelian and Proto-Afrasian with the traditional plain voiceless stops of Proto-Indo-European. *This reconstruction would make the glottalized stops the least marked members of the Proto-Nostratic stop system. Illič-Svityč's and Dolgopolsky's reconstruction is thus in contradiction to typological evidence, according to which glottalized stops are uniformly the most highly marked members of a hierarchy.* The reason that Illič-Svityč's and Dolgopolsky's reconstruction would make the glottalized stops the least marked members is as follows: Illič-Svityč and Dolgopolsky posit glottalics for Proto-Nostratic on the basis of one or two seemingly solid examples in which glottalics in Proto-Afrasian and/or Proto-Kartvelian appear to correspond to

> traditional plain voiceless stops in Proto-Indo-European. On the basis of these examples, they assume that, whenever there is a voiceless stop in the Proto-Indo-European examples they cite, a glottalic is to be reconstructed for Proto-Nostratic, even when there are no glottalics in the corresponding Kartvelian and Afrasian forms! This means that the Proto-Nostratic glottalics have the same frequency distribution as the Proto-Indo-European plain voiceless stops (Alexis Manaster Ramer 1997:94—95 makes the same observation [see below]). Clearly, this cannot be correct. The main consequence of the mistaken comparison of the glottalized stops of Proto-Kartvelian and Proto-Afrasian with the traditional plain voiceless stops of Proto-Indo-European is that Illič-Svityč and Dolgopolsky are led to posit forms for Proto-Nostratic on the basis of theoretical considerations but for which there is absolutely no evidence in any of the Nostratic daughter languages.

Starostin astutely observed that this is simply not correct as worded:

> It turns out that the "most highly marked" principle only works in the case of the labial stop — in *every* other instance glottalized consonants are obviously the *least* highly marked ones in the system!

Starostin provides a chart showing the frequency distribution of glottalics in Proto-Kartvelian as an example (see below). Consequently, I have corrected the paragraph so that it now reads (the corrected wording is in italics):

> The mistake that Illič-Svityč and Dolgopolsky made was in trying to equate the glottalized stops of Proto-Kartvelian and Proto-Afrasian with the traditional plain voiceless stops of Proto-Indo-European. *Their reconstruction would make the glottalized stops the least marked members in the Proto-Nostratic labial series and the most marked in the velar series. Such a reconstruction is thus in contradiction to typological evidence, according to which glottalized stops uniformly have the opposite frequency distribution (most marked in the labial series and least marked in the velar series [for details, cf. Gamkrelidze 1978]).* The reason that Illič-Svityč's and Dolgopolsky's reconstruction contradicts the typological evidence is as follows: Illič-Svityč and Dolgopolsky posit glottalics for Proto-Nostratic on the basis of a small number of seemingly solid examples in which glottalics in Proto-Afrasian and/or Proto-Kartvelian appear to correspond to traditional plain voiceless stops in Proto-Indo-European. On the basis of these examples, they assume that, whenever there is a voiceless stop in the Proto-Indo-European examples they cite, a glottalic is to be reconstructed for Proto-Nostratic, even when there are no glottalics in the corresponding Kartvelian and Afrasian forms! This means that the Proto-Nostratic glottalics have the same frequency distribution as the Proto-Indo-European plain voiceless stops (Alexis Manaster Ramer 1997:94—95 makes the same observation [see below]). Clearly, this cannot be correct. The main consequence of the mistaken comparison of the glottalized stops of Proto-Kartvelian and Proto-Afrasian with the traditional plain voiceless stops of Proto-Indo-European is that Illič-Svityč and Dolgopolsky are led to posit forms for Proto-Nostratic on the basis of theoretical considerations but for which there is absolutely no evidence in any of the Nostratic daughter languages.

As stated above, I am extremely grateful to Starostin for bringing this error to my attention, because correcting the wording to reflect what is actually found in natural languages having glottalics has had the unanticipated effect of strengthening my case.

The expected frequency distribution may be illustrated by the chart given by Starostin for Proto-Kartvelian (he based his counts on Klimov's *Etymological Dictionary of the Kartvelian Languages*):

Phoneme	#	Phoneme	#	Phoneme	#	Phoneme	#
*b-	83	*g-	50	*d-	44	*ʒ- + *ʒ́-	50
*p-	59	*k-	42	*t-	36	*c- + *ć-	63
*p'-	19	*k'-	106	*t'-	53	*c'- + *ć'-	112

In an important study on the hierarchical correlation of elements in a phonological system, Gamkrelidze (1978:9—46) has shown that stops and fricatives arrange themselves into definite hierarchical relationships based upon their relative frequency of occurrence. The more common, more usual, more frequent a sound, the *less* marked it is in relationship to other sounds, which are less common, less usual, less frequent, that is, *more* marked. The various hierarchies established by Gamkrelidze were arrived at by investigating the frequency distribution of sounds in a great number of languages. These hierarchical relationships are found to be characteristic of language in general and not language specific, the underlying reasons being phonetic — the distinctive features making up the unmarked sounds simply combine with each other into simultaneous bundles more easily than do the distinctive features making up marked sounds. Finally, Gamkrelidze notes that, when there are gaps or empty slots in a system, they invariably occur at the point of articulation of the most highly marked member in the hierarchy.

The following are three of the hierarchies established by Gamkrelidze:

Least Marked — Most Marked

(1) /b/ → /p/ → /pʰ/ → /p'/
(2) /k'/ → /kʰ/ → /k/ → /g/
(3) /q'/ → /qʰ/ → /q/ → /ɢ/

The arrows indicate the direction of greater markedness. In the first hierarchy, /b/ is the most common, most usual, most frequent, hence least marked member; /p/ is less common than /b/ but more common than /pʰ/ and /p'/; /pʰ/ is less common than /b/ and /p/ but more common than /p'/; finally, /p'/ is the least common, hence, most marked member. Since gaps occur at the position of the mostly highly marked member, if there is a gap in this series, it will be /p'/ that will be missing. In the second hierarchy, on the other hand, the markedness relationship is reversed: /k'/ is the most common, most usual, most frequent, hence least marked member; /kʰ/ is less common than /k'/ but more common than /k/ and /g/; /k/ is less common than /kʰ/ and /k'/ but more common than /g/; finally, /g/ is the least common, hence, most marked member. Since gaps occur at the position of the mostly highly marked member, if there is a gap in this series, it will be /g/ that will be missing here. The postvelar series has the same markedness correlation as the velar series.

Here is Gamkrelidze's (1978:18 and 19) exact wording:

> The glottalized labial phoneme is functionally a weaker member than the voiceless aspirate, the latter being weaker than the simple voiceless phoneme.
>
> But united with the feature of velarity the feature of glottalization can also create a functionally stronger unit than aspiration can. In many systems the glottalized velar phoneme /k'/ has a greater frequency of occurrence than the corresponding aspirated or simple voiceless velar /k/; it is thereby the unmarked member of the opposition with respect to the unglottalized voiceless velar phoneme.
>
> In the group of voiceless postvelar stops the glottalized [post]velar stop /q'/ is the functionally strongest member; its frequency is greater than that of the corresponding aspirated phoneme /q^h/.

Compare the frequency distribution that Manaster Ramer found in Illič-Svityč's Nostratic dictionary:

> 6.1. Finally, quite recently, I decided to see what would happen if one counted up the occurrences of the different stops (voiceless vs. voiced vs. glottalized as well as labial vs. coronal vs. velar) reconstructed for Nostratic by Illich-Svitych. I only performed the experiment on root-initial stops, with the following results: (they are given as approximations because there is a problem arriving at exact figures given that there [are] some cases where it is difficult to tell whether one is dealing with a single Nostratic form or two, or whether a particular form should begin with this or that stop):

*b 50+	*d 20+	*g 40+
*p 15+	*t 15+	*k 50+
*p' 40+	*t' 30+	*k' 60+

> The first observation (see Manaster Ramer in press a) was that ... the relative frequencies of the three phonation types (voiced, voiceless, glottalized) posited for Proto-Nostratic stops, as reflected in the sets of cognates compiled by Illich-Svitych, seem to be inconsistent with typological predictions. Specifically, at least in initial position, the series of stops reconstructed as glottalized is much more frequent at all points of articulation than the series reconstructed as (plain) voiceless.

As can be seen, Manaster Ramer's findings, arrived at independently, confirm my own observations. The frequency distribution of stops in the system reconstructed for Proto-Nostratic by Illič-Svityč and Dolgopolsky is simply not in agreement with what is found in natural languages. Thus, it is highly suspect.

To his credit, Starostin acknowledges that:

> One thing over which I am definitely in agreement with Bomhard is that the work by pioneers of Nostratic should be subject to modification; nothing in the Nostratic theory as formulated by V. M. Illich-Svitych and A. Dolgopolsky should be considered "sacred", especially in the light of significant progress that has been achieved in the reconstruction of the daughter branches of Nostratic over the past half century. That said, it is hardly reasonable to introduce any kind of modifications to sound, stable systems unless demand for such modifications becomes truly overwhelming: "if it ain't broke, don't fix it".

But, does he really address the problems identified by myself and Manaster Ramer concerning the system reconstructed for Proto-Nostratic by Illič-Svityč and Dolgopolsky? The answer is "no, not really." As noted above, there are three separate issues involved here:

1. The traditional reconstruction of the Proto-Indo-European consonant system is flawed and is to be reinterpreted along the lines proposed by Thomas V. Gamkrelidze and Vjačeslav V. Ivanov and, independently, by Paul J. Hopper.

2. The frequency distribution of Proto-Nostratic stops in the reconstruction proposed by Illič-Svityč and Dolgopolsky is in contradiction to typological predictions, and is, consequently, highly suspect. To paraphrase Starostin, it is broken, and it does need to be fixed.

3. Taking into consideration (1) the radical reinterpretation of the Proto-Indo-European consonant system proposed by Gamkrelidze, Ivanov, and Hopper, as well as (2) the problems in the frequency distribution of stops in the reconstruction of the Proto-Nostratic phonological system proposed by Illič-Svityč and Dolgopolsǩy, a different set of Nostratic sound correspondences is warranted.

The main focus of Starostin's paper is an examination of the data used to support the alternative set of sound correspondences I have proposed, that is, to point number 3 above. Therefore, let us now turn our attention this topic.

In comparing the lexical material from the various Nostratic daughter languages, I have tried to be very careful about the issue of semantic plausibility. Where there is either a one-to-one or an extremely close semantic correspondence, there is, of course, no problem, and Starostin accepts such etymologies. Unfortunately, things are not always this straightforward. Quite often, there is not a one-to-one semantic correspondence — in such cases, we must be able to derive the proposed cognates from the postulated ancestor form by widely-attested semantic shifts and not by mere speculation. Therefore, in attempting to determine whether or not particular lexical items from the various language families under consideration might be related, I have made extensive use of Carl Darling Buck's *A Dictionary of Selected Synonyms in the Principal Indo-European Languages* as a control for the semantic development of the proposed lexical parallels, and references to the appropriate sections of this work are given at the end of each proposed Nostratic etymology. It may be noted that, in examining the lexicons of Kartvelian, Afrasian, Uralic-Yukaghir, Elamo-Dravidian, Altaic, and Eskimo-Aleut, semantic shifts similar to those described by Buck for the Indo-European languages are found over and over again in these other language families as well. I cannot emphasize strongly enough that, in order to gain a complete understanding of how I arrived at my proposals, Buck's dictionary must be consulted. However, in a number of instances, where I felt it was warranted due to the wide semantic differences found among the forms cited from the daughter languages, I have given brief explanations within the etymologies themselves.

As valuable as Buck's dictionary is, however, it is not without its shortcomings. In the first place, as noted by Buck himself (1949:xii), the dictionary is not complete —

due to the nature of the material involved, Buck and his assistants had to be selective in choosing what to include. Next, the research upon which the dictionary is based is now well over half a century old. Therefore, Buck's dictionary must be supplemented by more recent scholarship. Unfortunately, however, this work is spread throughout so many journal articles, dissertations, and books that it is virtually impossible to consult all of it, especially when one is dealing with multiple language families.

It is not enough, moreover, simply to compare dictionary forms. Rather, when working with the lexical data from the various Nostratic daughter languages, one must strive to ascertain the underlying semantics, that is to say, the fundamental meaning or meanings from which the full set of semantic nuances are derived, based upon actual usage, wherever this is possible, and one must be cognizant of the interrelationship between social, cultural, and conceptual factors on the one hand and semantic structures on the other. It goes without saying that this is neither a simple nor an easy undertaking.

The approach that I have followed thus leads to the establishment of what may be called "families of words" in the tradition of the great Indo-European comparative dictionaries such as Pokorny (1959) and Walde (1927—1932). The starting point is always the reconstructed Nostratic forms. The material cited from the individual Nostratic daughter languages is then to be judged primarily by whether or not it can be convincingly derived from the antecedent Nostratic forms either directly or through widely-attested semantic shifts.

The difficulties involved in dealing with semantic change in unattested languages have been clearly articulated by Winter (2003:206—207):

> The difficulties encountered by the etymologist reaching out toward unattested and hence textless languages are deep-rooted and virtually insurmountable. When one reads an intelligently written, richly documented book such as Blank 1997, one cannot help being impressed with the fact that it is relatively easy to describe and classify semantic changes such as metonymy and metaphor that occurred in the course of the history of an individual language or a group of closely related languages, but that there is a near total absence of what one might call determinacy and hence predictability as to the kind and direction of the changes that can be shown to have taken place and therefore can be expected to occur again under comparable circumstances. The applicability of a form may remain unchanged, it may be extended to cover additional meaning configurations, it may be narrowed, it may be eliminated altogether; new denotations added may concern closely related items, as in the case of metonymy, or seemingly very different ones, as when metaphors are used. As long as one limits oneself to a retrospective analysis of data from historically well-attested languages, the lack of regularity will not affect the descriptive adequacy of one's findings; if, however, one turns to the study of prehistoric stages of a language or a group of languages, one is left with hardly any well-defined criteria by which to evaluate one's hypotheses (and those of others). The only criterion that seems to be operationally usable derives from the assumption made above: if both phonetic and semantic change occurred in relatively small steps, feature by feature, component by component, then the likelihood that a hypothesis might be correct can be said to be supported in a more than subjective way. This does not eliminate the difficulty that observable change can occur in all possible directions and that to complicate matters even further, in the course of a historical development, the direction may change at any time. If that is the case, it follows that in the absence of observable data — that is, under

> conditions normal for reconstructional linguists — no objectifiable criteria can be called upon by the etymologist, and his proposals will always reflect his personal preferences.

As reported in the 11 October 2007 issue of *Nature*, a new study by a research team headed by the evolutionary biologist Mark Pagel of the University of Reading in England found that the frequency of word usage exerts a "lawlike" influence on the rate of vocabulary change. The research team applied a statistical analysis to 200 basic vocabulary items in various Indo-European daughter languages in order first to determine the frequency of usage and then to determine the stability of various words over time. They found that the most frequently used words, such as numbers, pronouns, and so-called "special adverbs" such as "who," "what," "where," "when," "how," and "not" changed the least. Words such as these undergo no more than one wholesale shift to a new form every 10,000 years. In contrast, less frequently used words, such as "dirty," "turn," "guts," etc., changed more rapidly. Such words changed forms up to nine times every 10,000 years. This study provides important insights into vocabulary change. No doubt, the use of sophisticated statistic methods such as this will provide a useful tool to help quantify how words change over time.

Another important point that needs to be made here concerns how I segment the reconstructed forms I propose. Comparison of the various Nostratic daughter languages indicates that the rules governing the structural patterning of roots and stems is Proto-Nostratic were most likely as follows:

1. There were no initial vowels in Proto-Nostratic. Therefore, every root began with a consonant.
2. Originally, there were no initial consonant clusters either. Consequently, every root began with one and only one consonant. Medial clusters were permitted, however.
3. Two basic root types existed: (A) **CV* and (B) **CVC*, where *C* = any non-syllabic, and *V* = any vowel. Permissible root forms coincided exactly with these two syllable types.
4. A stem could either be identical with a root or it could consist of a root plus a single derivational morpheme added as a suffix to the root: **CVC+CV-*. Any consonant could serve as a suffix.
5. A stem could thus assume any one of the following shapes: (A) **CV-*, (B) **CVC-*, (C) **CVC+CV-*, or (D) **CVC-CVC-*. As in Proto-Altaic, the undifferentiated stems were real forms in themselves and could be used without additional suffixes or grammatical endings. However, when so used, a vowel had to be added to the stem (unless the stem already ended in a vowel or in a semivowel, nasal, or liquid), thus: (A) **CV-* > **CV* (no change), (B) **CVC-* > **CVC*+V, (C) **CVC-CV-* > (no change), or (D) **CVC-CVC-* > **CVC-CVC*+V. Following Afrasian terminology, this vowel may be called a "terminal vowel" (TV). Not only did terminal vowels exist in Proto-Afrasian, they were also found in Dravidian, where they are called "enunciative vowels". As in Proto-Dravidian, the terminal vowel was only required in stems ending in obstruents, which could not occur in final position.

The original root structure patterning was maintained longer in Afrasian, Dravidian, and Altaic than in the other branches, while the patterning found Proto-Indo-European and Proto-Kartvelian has been modified by developments specific to each of these branches. The root structure constraints found in Proto-Indo-European were an innovation. In Proto-Uralic, the rule requiring that all words end in a vowel was an innovation and arose from the incorporation of the so-called "terminal vowel" into the stem. It should be mentioned here that reduplication was a widespread phenomenon.

On the basis of the evidence of Proto-Indo-European, Proto-Kartvelian, Proto-Afrasian, Proto-Dravidian, and Proto-Altaic, it may be assumed that there were three fundamental stem types: (A) verbal stems, (B) nominal and adjectival stems, and (C) pronominal and indeclinable stems. Some stems were exclusively nominal. In the majority of cases, however, both verbal stems and nominal stems could be built from the same root. In Proto-Nostratic, only pronominal and indeclinable stems could end in a vowel. Verbal and nominal stems, on the other hand, had to end in a consonant, though, as noted above, when the undifferentiated stems were used as real words in themselves, a "terminal vowel" had to be added to the stem (but only when the stem ended in an obstruent). The terminal vowels were morphologically significant. Adjectives did not exist as an independent grammatical category in Proto-Nostratic.

As can be seen, I have tried to eliminate the arbitrary nature of much of the previous work, as well as some current work, in lexical comparison by relying heavily on proven, widely-attested semantic shifts as found in the daughter languages, especially Indo-European, Semitic, and Dravidian, which, due to having written records of sufficient time depth to be able to follow how words have changed meaning over time, as well as due to having voluminous data with which to work, are particularly valuable. My approach is thus positivistic, that is, data-oriented, rather than impressionistic. Moreover, I supply a large amount of cited forms from the daughter languages to illustrate the types of changes that have occurred, I give explanations where needed, I supply voluminous references to the standard etymological dictionaries and other relevant literature, I set rather narrow limits on the meanings of the terms selected for comparison, and I stay well within the bounds of established scholarship within each language family. Inasmuch as not a single one of my proposals is impressionistic or arbitrary or based upon speculation or personal preference or preconceived notions in terms of semantic plausibility, I must reject Starostin's conclusions in total, since they appear to me to be based more on an impressionist evaluation (that is, they are based on personal preference) than on actual examination of the supporting data. Thus, I believe that it is not proper to challenge the Nostratic etymologies that I have proposed on the grounds of *semantic plausibility* or *lack of plausibility*, as Starostin has done. *They are all plausible*, that is, they all can be shown to have documented parallels in attested languages. The only place where my proposals are vulnerable, in terms of semantic plausibility, is that I cannot *prove* that the developments I am suggesting actually happened the way I envision. Given that there are no written records going back that far, nobody can prove or disprove this. One final point needs to be made here — we must be careful not to confuse *plausibility* with *reliability*. Even though all of my proposals are *plausible*, it must be acknowledged that some of the etymologies I have proposed are more *reliable* than others, because the supporting evidence from the daughter languages is stronger, both in terms of quantity and quality — that is to say, more forms have survived in more daughter languages, and/or the original meanings have been preserved with greater accuracy in the surviving forms.

Let us end this discussion by making two final points:

1. To my knowledge, Starostin has not seen the full supporting data that accompany each etymology given in my paper. *All of the data I have supplied in my book must be taken into consideration in order to arrive at a fair and balanced evaluation of my views.* Not only has Starostin not seen the full supporting data for the examples involving glottalized stops in initial position, he has not seen any of the etymologies involving glottalized stops in non-initial position or any of the etymologies involving glottalized affricates. Moreover, the changes that I have proposed affect not only the glottalized stops and affricates, they also affect the voiceless (aspirated) stops and affricates. Altogether, there are literally hundreds upon hundreds of etymologies supporting my proposals — the volume of my book devoted to comparative vocabulary runs just over 900 pages in length. These remarks notwithstanding, I believe that Starostin has tried to give a fair and balanced assessment of my work.

2. Those, such as myself, Illič-Svityč, and Dolgopolsky, who are venturing into uncharted territory, must be particularly careful not to propose anything that is not characteristic of language in general, including semantic change. Each time that we fail to adhere to the most stringent methodological standards, we open ourselves to well-justified criticism. It should be obvious to all that I have tried to adhere to such standards.

MOTHER TONGUE
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On the Application of Glottochronology to Kartvelian Languages

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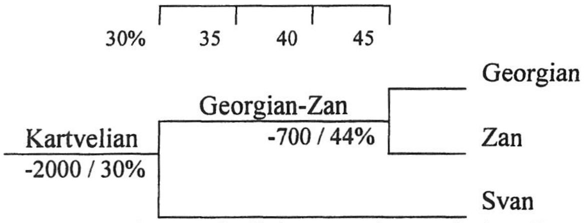
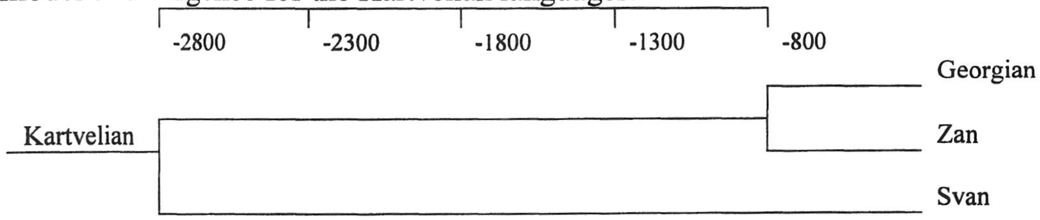
1.1. The first attempt to apply glottochronology to Kartvelian languages was published by G.A. Klimov (1961, 243, 245). His results based on the 'classical' method of Swadesh are as follows:

language	Zan	Svan
Georgian	44%	30%
Zan		30%

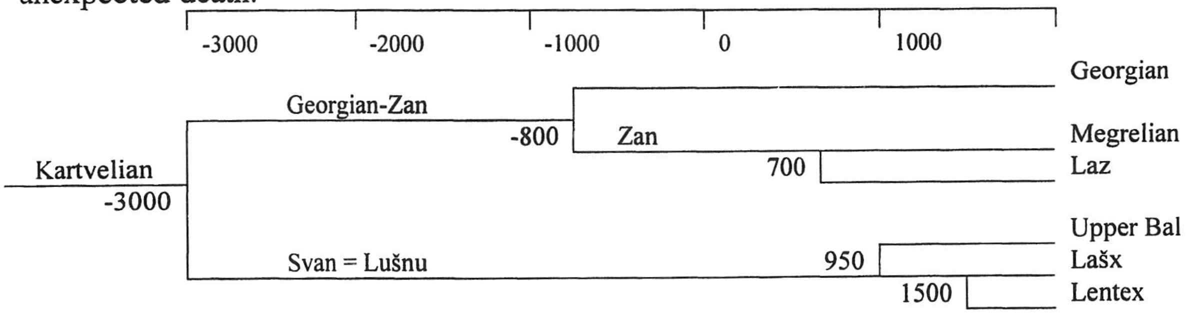

30%
35
40
45
Georgian
Georgian-Zan
Kartvelian
-700 / 44%
Zan
-2000 / 30%
Svan

Note: Later Klimov (Kl. = 1998, ix) postponed the time of disintegration of the Svan and Georgian-Zan protolanguages to 2200 BC.

1.2. Applying the 'recalibrated' method of Sergei Starostin, Testelec (1995, 14) proposed the following model of divergence for the Kartvelian languages:


-2800
-2300
-1800
-1300
-800
Georgian
Kartvelian
Zan
Svan

1.3. The most detailed scheme illustrating the disintegration of the Kartvelian languages was realized by Sergei Starostin and his team. The following diagram, probably never published, was kindly offered by Sergei Starostin to V. Blažek in June 2005, 3 months before Starostin's unexpected death:


-3000
-2000
-1000
0
1000
Georgian
Georgian-Zan
-800
Zan
Megrelian
Kartvelian
700
Laz
-3000
Upper Bal
Svan = Lušnu
950
Lašx
1500
Lentex

2. The purpose of the present study is to collect the basic 100-word-lists for 4 modern Kartvelian languages, namely Georgian, Megrelian, Laz and Svan, plus Old Georgian, to analyze etymologically their lexics, and finally to calculate the pecentages of mutual cognates. The final

results should serve as a base for their own tree-diagram, which will be compared with the previous ones.

3. Statistical survey of the word-lists (only such loans are taken in account, which are attested as exclusive representants of the corresponding semantic unit):

Georgian:
Lacks: ø
Loans: 8, 27b, 52a, 58c, 90b.

Megrelian:
Lacks: 1.
Loans: 3a, 6a+b, 15a, 24a, 35a, 46b, 59a, 69a, 100a.

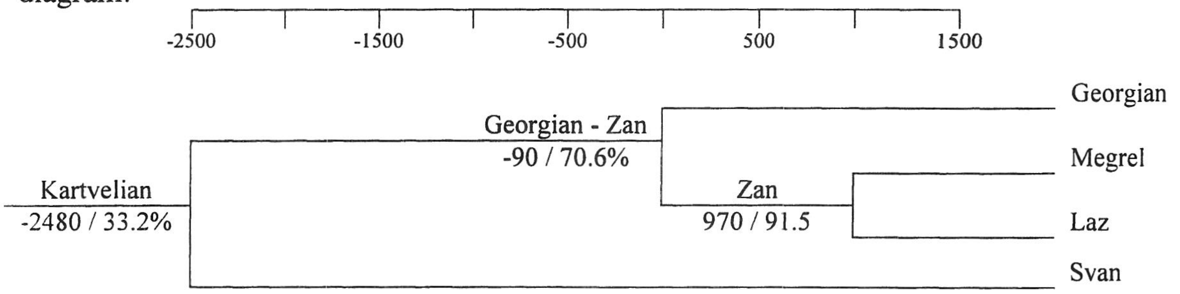
Laz:
Lacks: 1, 4, 14, 26, 78.
Loans: 3a, 35b, 48d, 69a, 70c, 100b.

Svan:
Lacks: ø.
Loans: 65b, 68b, 70a, 93b, 100a.

Our results are summarized into the **Table 1**:

	Megrelian	Laz	Svan
Georgian	62/85 = 72.9%	58/85 = 68.2%	30/90 = 33.3%
Megrelian		75/82 = 91.5%	29/85 = 34.1%
Laz			27/84 = 32.1%

These figures projected into the absolute chronology allow us to construct the following tree-diagram:



Gloss	Georgian	Old Georg.	Megrelian	Laz	Svan	Etymology / Source
1a. all	*q̇vela* (*q̇oveli* every)	*q̇oveli*	(*ir-* every)	(*ir-* every)		GZ **q̇owl-* (Kl. 244)
1b. all					*mäg*	
2a. ashes	(*ṭuṭa-* alkali)		*ṭuṭa-*	*mṭuṭa-*	*ṭəṭ(a)-*	K **ṭuṭa-* (Kl. 192)
2b. ashes	*nacari*	*nacari*				(KX 199)
2c. ashes	(*mṭveri* dust)	*mṭueri*	(*ṭveri* dust)	*mṭveri*		GZ **mṭwer-* (Kl. 126; KX 199)

2d. ashes					*käm*	(KX 199)
2e. ashes	(*γvelpi* hot ashes)	(*γvelpi* id.)	(*γvalp-* id.)	(*γvalpu-* soot)	*q̇welp*	K **q̇welp-* (Kl. 240)
3a. bark	*kerki*	*kerki*	*kerki* < G. (*karkalaḳi* kindle-wood)	*kerki* < G.		(KX 215; F 16)
3b. bark					*cil*	(KX 215)
4a. belly	*muceli*	*muceli*				
4b. belly	*ṗaγv-*		(*ṗuγu-* humpback)			GZ **ṗaγw-* (Kl. 148)
4c. belly			*kora*			
4d. belly					*qäd*	
5a. big	*didi*	*didi*	*didi*	*didi*		GZ **did-* (Kl. 40)
5b. big					*xoša*	(KX 305)
5c. big	(*sr-ul-i* whole)				*swril*	K **sur-* complete (FS 305)
5d. big					*ʒγed*	(KX 305)
6a. bird	*prinveli*	*mprinveli*	*pruveli* < G.		*näpr & memp(w)er*	cf. S *li-per* to fly (KX 241-42)
6b. bird	*čiṭi*		*čiṭi* < G.			(KX 241-42)
				ḳ(w)inči		(KX 241)
					päpol	
7a. bite	*ḳbena*	*ḳbena*	*ḳibirua*	*o-ḳibin-u*		**ḳb-en-* / **ḳb-in-* (Kl. 87)
7b. bite			*čamini*			(KX 411)
7c. bite				*meč̣ḳom*		cf. #23a (KX 411)
7d. bite					*li-qep*	(KX 411)
8a. black	*šavi*	*šavi*				< Osset. (KX 354)
8b. black			*uča*	*uča*		(KX 354)
8c. black					*mešxe*	*burnt; cf. #12b: S *šx-* to burn (Kl. 274)
9. blood	*sisxli*	*sisxli*	*zisxiri*	*di(n)cxiri*	*zisx*	K **zisxl-* (Kl. 59)
10a. bone	*ʒvali*	*ʒuali*	*dvali* < G		*ǯiǯw*	K **ʒ$_1$ʒ$_1$wal-* (Kl. 291)
10b. bone	(*q̇vlivi* bone of shoulder)		*^{c}vil(e)-*	*q̇wili/^{c}ili*		GZ **q̇wl-iw-* (Kl. 242)
11a. breast	*mḳerdi*	*mḳerd-*	*ḳədəri*		*məč̣wed*	G **mḳerd-* / **mḳrd-* (Kl. 123)
11b. breast				*gurpiǯi*		(KX 72)
11c. breast	(*gvami* corpse)	(*guami* body)			*gwäm*	K **gwam-* (Kl. 28)
12a. burn	*c̣va*	*c̣ua*	*č̣uala*	*o-č̣-u*	(*č̣-* bake)	K **c̣$_1$w-* (Kl. 310)
12b. burn	*cx-*	*cx-*	(*cxana* heat)	(*do-čx-u* become hot)	*šx-*	K **c$_1$x-* (Kl. 274)
13. claw	*prčxili*	*prcxili*	*bircxa*	*bu(r)cxa*	*cxa*	K **prcxa-* (Kl. 204-05)
14a. cloud	*γrubeli*	*γrubeli*				
14b. cloud			*munapa*			
14c. cloud					*mēre*	
15a. cold	*grili*	*grili*	*rgili* < G			(KX 352-53)
15b. cold	*civi*	*civi*				(KX 352-53)
15c. cold				*q̇ini / ʔini*		K **q̇in-* to cool, freeze (Kl. 243)
15d. cold					*məcxi*	(KX 352)
16a. come	*mosvla*	*moslva*	*mu(u)la*			cf. #92b
16b. come	(*xad-* : *xd-* to take)	(*qad-* : *qd-* id., go)	(*rt-* to go)	*mo-xt-im*	*li-qed*	K **qed-* : **qd-* (Kl. 335)
17a. die	*ḳved-/ ḳvd-*	*ḳud-*			(*ḳwäd* loss)	K **ḳwed-* : **ḳwd-* (Kl. 91)
17b. die			*γurua*	*o-γur-u*		(KX 450)
17c. die					*li-šgr-e*	(KX 450)
18a. dog	*ʒaγli*	*ʒaγli*	*ǯoγori*	*ǯoγo(r)i*	*žeγ / žaγ*	K **ʒ$_1$aγl-* (Kl. 283)
18b. dog	(*leḳvi* puppy)	(*leḳui* puppy)	*laḳvi*	*laḳ-/lač̣-*		GZ **leḳw-* (Kl. 108)

19a. drink	_sv-_	_su-_	_š(v)-_	_š(v)-_	_š(w)-_	K _*s$_1$w-_ (Kl. 179)
19aa. drink	_svam-/ sm-_	_svam-/ sum-_	_šum-_	_šum-_		GZ _*s$_1$w-am- / *s$_1$w-m-_ (Kl. 179)
19b. drink	(_tver-/ tr-_ get drunk)				_tr-_	K _*ter- / *tr-_ (Kl. 69)
20a. dry	_xmeli_	_qmeli_	_xumla-_	_xom(b)ula-_		GZ _*qm-el-_ (Kl. 339) : _*qem-/*qm-_ to dry (Kl. 335)
20b. dry					_puḳwi_	(KX 346)
21a. ear	_q̇uri_	_q̇uri_	^{c}_už-_	_(q̇)už-/_^{c}_už-_		GZ _*q̇ur-_ (Kl. 246)
21b. ear					_šdim_	(KX 100)
22a. earth	_mic̣a_	_mic̣a_				(KX 198)
22b. earth	_niadagi_					(KX 198)
22c. earth	(_tixa_ clay)	(_tiqa_ id.)	_dixa_	_(n)dixa_		K _*tiqa-_ (Kl. 72)
22d. earth					_gim_	(KX 198)
22e. earth					_wer_	(KX 198)
23a. eat	_č̣am-_: _č̣m-_	_č̣am-_	_č̣ḳom-_	_č̣ḳom-_	_ēm-_	K _*č̣am- / *č̣m-_ (Kl. 319)
23b. eat	(_sa-mxar-_ dinner)	(_sa-mxar-_ dinner)		_m(p)xor-_		GZ _*mxar-_ (Kl. 134)
24a. egg	_ḳvercxi_	_ḳuercxi_	_kvercxi_ < G			(KX 190)
24b. egg				_ma(r)kvali_		(KX 190)
24c. egg					_lə-gr-e_	from K _*gr-_ lay eggs (Kl. 32)
25. eye	_tvali_	_tuali_	_tol-_	_tol-_	_te(r)_	K _*twal-_ (Kl. 70)
26a. fat n.	_cximi_				_mə-čxim_	K _*c$_1$xim-_ (Kl. 276)
26b. fat n.	_cmeli_	_cmeli_	(_cimu-_ scum of fat)	(_cimu-luγ-_ white fig)	_cemil_	K _*c(w)m-el-_ : _*cwem-/*cwm-_ to smear (Kl. 265)
26c. fat n.	_koni_	_koni_	_koni_	(_kun-/kin-_ brain.)		GZ _*kon-_ (Kl. 218)
26d. fat n.					_γwäse_	(KX 180)
27a. feather	(_sve-_ wing)		_(p)sua_	_((m)p)sva_		GZ _*swe-_ (Kl. 165)
27b. feather	_prta_	_prte_				< IE? (KX 89)
27c. feather					_bīnṭq̇il_	(KX 89)
28a. fire	_cecxli_	_cecxli_	_dačxiri_	_dačxuri_		GZ _*ʒec$_1$xl-_ (Kl. 280)
28b. fire					_lemesg_	(KX 202)
29a. fish	_tevzi_	_tevzi_				(KX 242)
29b. fish	(_čxam-a_ small fish)		_čxomi_	_čxomi_		GZ _*čxam-_ (Kl. 259)
29c. fish					_cuz_	(KX 242)
29d. fish					_ḳalmax_	< G _ḳalmaxi_ trout (KX 242)
30a. fly v.	(_pepela_ butterfly)		(_perpalua_ move quickly)		_li-per_	K _*per-_ (Kl. 149)
30aa. fly v.	_pren-_: _prin-_	_prin-_	_purin-_	_purtin-_		GZ _*pr-en- / *pr-in-_ (Kl. 203)
31a. foot	_pexi_	_perqi_	(_la-bax-u_ passage)		(_bērq_ step)	GZ _*berq-_ (Kl. 12)
31b. foot		(_mḳwircxl-_ pedestrian)	_ḳučxi_	_ḳučxe_	_č̣išx_	K _*ḳwi(r)c$_1$x-_ (Kl. 95)
32a. full	_savse-_	_savse-_	_opša-/epša-_	_(j)opša-_		GZ _*(s)a-ws$_1$-e-_ (Kl. 173)
32ab. full			_gopša-_		_g(w)eši-_	(Kl. 174)
33a. give	_c-_	_c-_	_č-_	_č-_		GZ _*c$_1$-_ (Kl. 269)
33aa. give	_cem-_	_cem-_	_čam-_	(_čam-_ feed)		GZ _*c$_1$-em-_ (Kl. 270)
33b. give	(_c̣vd-_ grant)				_hod-_: _hwd-_	K _*c̣$_1$wed- / *c̣wd-_ (Kl. 310)
34a. good	_ḳetili_	_ḳetili_				(KX 353)
34b. good	_kargi_			_kai_ < G		cf. Arm. _ḳarg_ order (KX 353)
34c. good			_ǯgiri_			(KX 353)
34d. good				_vorsi_		good, nice (KX 353)
34e. good					_xoča_	(KX 353)
34f. good					_ezär_	(KX 353)

35a. green	*mçvane*	*mçuane*	*(r)cvane* < G			(KX 317)
35b. green				*ješil*		< Turk. *yeşil* (KX 317)
35c. green					*jərži*	(KX 317)
35d. green					*cwanil*	< G *mçvanili* green (n.) (KX 317)
36. hair	*tma*	*tma*	*toma*	*(n)toma*		GZ **tma-* (Kl. 73)
37a. hand	*xeli*	*qeli*	*xe-*, pl. *xel*	*xe-*	(*qä-l* arm, fathom)	K **qe-* (Kl. 334)
37b. hand					*ši*	(KX 96)
37c. hand					*ṭoṭ*	(KX 96)
38a. head	*tav-*	*tav-*	(*ti-ša* to himself)	*ti-*	(*šda* ear of cereals)	K **taw-* (Kl. 66)
38b. head	(*dudi* tip, crest)		*dudi*	*dudi*	(*dudūl* nipple)	K **dud-* (Kl. 42)
38c. head	(*txemi* back of the head)	(*txemi* top)			*txwim/txum*	K **txam-* (Kl. 77)
39. hear	*sm-*	*sm-*	*sim-*	*sim-*	*sm-/səm-*	K **sem-* : **sm-* (Kl. 163)
40. heart	*guli*	*guli*	*guri*	*gur-*	*gwi-/gu(h)-*	K **gul-* (Kl. 35)
41a. horn	*rka*	*rka*	*ka*	*kra*		GZ **kra* (KX 95)
41b. horn					*mič̣w*	(KX 95)
42. I	*me*	*me / men*	*ma*	*ma(n)*	*mi*	K **men* (Kl. 119)
43a. kill	*ḳvla*	*ḳlva*			(*č̣aw* tödliche Seuche)	K **ḳal-* / **ḳl-* (KX 449; FS 183)
43b. kill			*ʔvilua*	*o-q̇vil-u*		(KX 449)
43c. kill					*li-dgär-i*	(KX 449)
44a. knee	*muxli*	*muqli*	(*muxur* corner, edge)			GZ **muql-* (Kl. 127
44b. knee			*birguli*	*burgili*		(KX 80)
44c. knee					*kutūl*	(KX 80)
44d. knee					*γulaj*	(KX 80)
45a. know	*uçq̇-*	*uçq̇-*	(*orçq̇-* see)	(*oç(ḳ)-* look)		GZ **uḳq̇-* (Kl. 196)
45b. know	*ceb-*: *cb-*	*ceb-*			*kab-*: *kb-*	K **kab-* / **kb-* (Kl. 211)
45c. know	(*čan-*: *čn-* be seen)		*čkina* / *čkuna*	*o-čkin-u*		GZ **čan-* **čn-* (Kl. 254; KX 455)
45d. know	*codna*					(KX 455)
45e. know					*li-xal*	(KX 455)
46a. leaf	*potoli*					(KX 216)
46b. leaf	(*purceli* foliage)	*purceli*	*purceli* < G (*purča* husk)	(*purča* husk)		GZ **purc*$_1$*-el-* (Kl. 207; KX 216)
46c. leaf	(*buṭḳo-* inflorescence)			*buṭḳa*		GZ **buṭḳa-* (Kl. 21)
46d. leaf					*bale*	(KX 216)
47. lie	*dev-/d(v)-*	*dv-*	*d(v)-*	*d(v)-*	*d-*	K **dew-* / **dw-* (Kl. 39)
48a. liver	*γviʒli*	*γviʒli*	(*q̇vižil-* black-violet)		*q̇wiže/q̇uže*	K **q̇wiʒ*$_1$*-l-* (Kl. 242)
48b. liver			*čxončxi*			(KX 90)
48c. liver			*mangari*			(KX 90)
48d. liver				*ǯigeri*		< Turk. *ciğer* (KX 90)
49a. long	*g(r)ʒeli*	*grʒeli*	*gərʒa/e* ~ *gənʒa/e*	*ginʒe-* / *gunʒe-*	(*li-gʒ-ən-e* be long)	K **grʒ-el-* / **grʒ-* be long (Kl. 33)
49b. long					*ǯōdi*	(KX 314)
50. louse	*ṭili*	*ṭili*	*ṭi(j)-*	*mṭi(j)-*	*ṭiš*	K **ṭil-* (Kl. 188)
51a. man	*ḳaci*	*ḳaci*	*ḳoči*	*ḳoči*	(*č̣äš* husband)	K **ḳac*$_1$*-* (Kl. 86)
51b. man					*māre*	(KX 40)
52a. many	*bevri*		*breli* < G	*breuli* < G		< Iran.: MPers. *bēvār* 10000 (KX 361)
52b. many		*mravali*	*miaro*?			
52c. many				*dido*		cf. GZ **did-* big (Kl. 40; KX 361)

52d. many					*(w)ōbäj*	(KX 361)
52e. many					*xwäj*	(KX 361)
53a. meat	*xorci*	*qorci*	*xorci*	*xorc- xo(r)ç-*		GZ **qorc-* (Kl. 339)
53b. meat					*leγw*	(KX 183)
53c. meat	(*mʒori* carcass)				*ʒwer*	K **ʒor-* (F 17; KX 183)
54a. moon	*tve-*	(*twite, ttue* month)	*tuta*	*(m)tuta*	*došt-ul*	K **tute-* (Kl. 74); cf. K **twet-* white (#97a)
54aa. moon	*mtvare*	*mtovare*				
55a. mountain	*mta*					(KX 194)
55b mountain	(*gora* hill)	(*gora* hill)	*gvala/gola*			GZ **gora* (Kl. 31; KX 194)
55c mountain				*raḳani*		(KX 194)
55d mountain				*dayi*		< Turk. *dağ* (KX 194)
55e. mountain					*tanaγ*	(KX 194)
55a. mountain					*zagär*	(KX 194)
56a. mouth	*ṗiri*	*ṗiri*	*ṗiǯi*	*ṗiǯi*	(*ṗil / bil* lip, edge)	K **ṗir-* (Kl. 150-51)
56b. mouth	(*xaxa-* pharynx)	(*qaqa-* id.: Orbeliani)			*qarq*	K **qarqa-* (Kl. 334; KX 95-96)
56c. mouth					*wišḳw*	(KX 95-96)
57. name	*saxeli*	*saxeli*	*ǯoxo-*	*ǯoxo-*	*žaxe*	**ʒ*$_1$*ax-e-*; cf. **ʒ*$_1$*ax- / *ʒ*$_1$*x-* to call (Kl. 283)
58a. neck	(*q̇eli* throat)	*q̇eli*	*ˤeli*	*(q̇)ali/ˤali*	*mə-q̇l-a*	K **q̇el-* (Kl. 238)
58b. neck	(*kedi* back of head)	*kedi*	(*kind-ir-* back of head)			GZ **ked-* (Kl. 214)
58c. neck	*kiseri*		*kiseri* < G			< Iran. (KX 102)
58d. neck					*ḳīnčx*	(KX 102)
58e. neck					*q̇ija*	(KX 102)
59a. new	*axali*	*axali*	*axali* < G			(KX 333)
59b. new				*aγani, aγne*		(KX 333)
59c. new					*maxe*	(KX 333)
60a. night	*γame*	*γame*	(*γuma* last night)	(*γoman* yesterday)		GZ **γame-* (Kl. 220; KX 284)
60b. night			*seri*	*seri*		(KX 284)
60c. night					*lēt*	(KX 284)
61a. nose	*cxviri*	*cxviri*	*čxvindi*	*čx(v)indi*		GZ **c*$_1$*xwir-* (Kl. 275)
61b. nose					*nepxwna*	(KX 88)
62a. not	*ver*	*ver*	*va(r)*	*va(r)*		GZ **wer* (Kl. 51)
62b. not				*mo(t)*	*mād(e)*	K **mad* (Kl. 113)
62c. not	*nu*	*nu*	*nu*		*nō-ma*	K **nu* (Kl. 144)
63a. one	*erti*	*erti*	*arti*	*ar(t)-*		GZ **ert-* (Kl. 47)
63b. one	(*sxva* other)	(*sxua* id.)	(*šxva* id.)	(*čkwa* id.)	*ešxu*	K **s*$_1$*xwa* (Kl. 184)
64a. person	*ḳaci*	*ḳaci*	*ḳoči*	*ḳoči*	(*č̣äš* husband)	= #51a
64b. person					*māre*	= #51b
65a. rain n.	*ċvima*	*ċvima*	*č̣vima*	*(m)č̣(v)ima*		GZ **ċwim-a-* : **ċwim-* to rain (Kl. 312)
65b. rain n.					*učxa*	< Adyghean *wəšxɨ* (KX 253)
66a. red	*ċiteli*	*ċiteli*	*č̣ita-*	*(m)č̣ita-*		GZ **ċ*$_1$*it-el-* : **ċit-* become red (Kl. 313)
66b. red	(*ċirani* dark red)				*ċərni*	(KX 320)
67a. road	*gza*	*gza*	*za*	*(n)gza*		GZ **gza* (Kl. 30)
67b. road					*šuḳw/šuḳuld*	(KX 196-97)
67c. road	*šara*	*šara*	*šara* < G	*šara-gza* < G	*šara* < G	< Pers. *šahra* (KX 196)
68a. root	*pesvi*	*pesui*	*pos(ve)*	*poso*		GZ **pesw-* (Kl. 200)
68b. root	*ʒiri*	*ʒiri*	*ǯinǯi*	*ǯiǯi*	*ʒir* < G	GZ **ʒ*$_1$*ir-* (Kl. 288)
68c. root				*k'oki*		< Turk. *kök* (KX 215)

69a. round	_mrgvali_	_mgrguali_	_rgvali_ < G	_morgvali_ < G		(KX 321) < _*m-grgw-al-_ : K _*gor-_ / _*gr-_ to roll, wallow (Kl. 31-32)
69b. round					_ḳurpi_	(KX 321)
69c. round					_ḳəreṭ_	(KX 321)
70a. sand	_kviša_	_(m)kviša_	_kviša_ < G		_kwiše_ < G	(KX 204)
70b. sand	_sila_		_psila_			GZ _*sila_ (KX 204)
70c. sand				_kumi_		< Turk. _kum_ (KX 204)
71a. say	_tkv-_	_tku-_	_tk(v)-_	_tk(v)-_	_kw-_	K _*tkw-_ (Kl. 75)
71b. say	_rkv-_	_rku-_			_rekw-_	K _*rekw-_ / _*rkw-_ (Kl. 156)
71c. say					_li-ql-e_	(KX 483)
72a. see	_ʒin-_	_ʒin-_	_ǯin-_			GZ _*ʒ$_1$in-_ (KL. 287)
72b. see	_xedva_					(KX 453)
72c. see			_ʒirapa_	_o-ʒir-u_		(KX 453)
72d. see					_li-sgd-i_	(KX 453)
72e. see					_li-c̣ed_	(KX 454)
73a. seed	_tesli_	_tesli_	_tasi_	_tasi_		GZ _*tes-l-_ : _*tes-_ to seed (Kl. 69-70)
73b. seed				_č̣ḳemi_		(KX 158)
73c. seed					_lǟši_	(KX 157)
73d. seed					_gimaš_	(KX 158)
74a. sit	_das(x)ma_					(KX 439)
74b. sit			_xunapa_	_o-xun-u_		(KX 439)
74c. sit					_li-sgvr-_	
75a. skin	_ḳani_				_ḳän_ < G	(KX 79)
75b. skin	_ṭq̇avi_	_ṭq̇avi_	_ṭq̇ebi_	_ṭḳebi_		GZ _*ṭq̇aw-_ (Kl. 192)
75c. skin			_škembe_	_škembe_		(KX 79)
75d. skin					_ṭup_	(KX 79)
76a. sleep	_lul-_	_rul-_	_lur-_ / _rul-_	(_lir-_ doze)		GZ _*lul-_ (Kl. 110)
76b. sleep	_ʒin-_	_ʒin-_	(_ǯir-_ lie)	_ǯin-_ / _ǯir-_		GZ _*ʒ$_1$in-_ (Kl. 287)
76c. sleep					_li-wž-e_	(KX 444)
77a. small	_c̣vrili_	_(m)c̣ulili_, _c̣urili_		_c̣ulu_		GZ _*c̣wl-il-_ (Kl. 295)
77b. small	(_c̣iku_ smallest)		_čiḳu_			GZ _*c̣$_1$iḳu-_ (Kl. 313)
77c. small	_c̣uḳ-_		(_č̣uḳ-_ mouse)			GZ _*c̣$_1$uḳ-_ (Kl. 316)
77d. small	_ṗaṭara_					< Arm. _paṭar_ piece (KX 325)
77e. small	(_coṭa_ few)		_č̣ič̣e_	_č̣iṭa_ / _č̣uṭa_		GZ _*c$_1$oṭa-_ (Kl. 273; KX 324-25)
77f. small	(_ḳuṭa_ boy)		(_ḳuṭu_ penis pueri; _ḳuṭula_ boy)		_ḳoṭōl/ḳoč̣ol_	K _*ḳuṭu_ (Kl. 105; KX 324)
78a smoke n.	_ḳvamli_	_ḳuamli_	_ḳuma_		_ḳwām_	K _*ḳwam-l-_ from _*ḳwam-_ / _*ḳwm-_ to smoke (Kl. 91)
78b. smoke n	_boli_					(KX 197)
78c. smoke n			_purḳi_			(KX 197)
79. stand	_dek-_ / _dg-_	_dg-_	_dg-_	_dg-_	_g-_	K _*deg-_ / _*dg-_ (Kl. 38)
80a. star	_varsḳvlavi_	_varsḳulavi_				(KX 198)
80b. star			_muricxi_	_murucxi_		(KX 198)
80c. star					_anṭq̇wasg_	(KX 198)
81a. stone	_kva_	_kva_	_kua_	_(n)kva_		GZ _*kwa_ (Kl. 215)
81b. stone					_bäč_	(KX 200)
82. sun	_mze_	_mze_	_bža_	_(m)žora-_ / _bžora-_	_məž_, _miž_	K _*mz$_1$e_ (Kl. 121)
83a. swim	_cur-_	_cur-_	_(n)čur-_	_nču(r)-_ / _mčvi(r)_		GZ _*c$_1$ur-_ (Kl. 273-74)
83b. swim					_li-lcun-e_	(KX 422)

84. tail	*ḳudi*	*ḳudi*	*ḳudeli*	*ḳudeli*	*(h)aḳwed*	K **ḳud-* : **ḳwed-* (Kl. 103)
85. that .	*i-/i-ma-/i-s-*	*i-ma-/i-s*	*i-/i-mu /* (*išo* there)	*i-/(h)i-mu-*	*i-/* (*im-eg* there)	**i-* / **i-ma-* /**i-s*$_1$- (Kl. 80-82)
86a. this	*ama-*		*amu*	*(h)amu*	(*ama-s* to that)	**a-ma-* (Kl. 2-3)
86b. this					*a, ala*	
87a. thou	*šen*	*šen*	(*skan* your)	(*skan/ckan* your)	(*isgwi* your)	K **š(w)en-* your (Kl. 250)
87aa. thou			*si*	*si(n)*	*si*	K **sen* (Kl. 164)
88. tongue	*ena*	*ena*	*nina*	*nena*	*nin, nən*	K **nena* (Kl. 141)
89. tooth	*ḳbili*	*ḳbili*	*ḳibiri*	*ḳibi(r)- /* *ḳirb-/č̣ibr-*		GZ **ḳb-il-* : **ḳb-(en-)* to bite (Kl. 87)
90a. tree	(*ʒeli* bar)	*ʒeli*	*ǯa-*, pl. *ǯal*	*(n)ǯa-*		GZ **ʒ*$_1$*el-* (Kl. 285)
90b. tree	*xe*					< Nakh: Ingush *χi* (KX 212)
90c. tree					*megem*	(KX 212)
91. two	*ori / vori*	*ori*	*žiri*	*žu(r)-/* *ǯu(r)-/jur-*	*jöri*	K **jor-* (Kl. 144)
92a. walk	*ved- : vid-*	*vid-*	*id-*	*id-*		GZ **wed-* / **wid-* (Kl. 51)
92b. walk	*svla-*	*slva*	*ula-*	*ulva-*		GZ **wl-a-* (Kl. 54)
92c. walk					*lī-z-i/li-zāl*	(KX 451)
93a. warm	*ṭbili*	*ṭpili*	*ṭibu-*	*ṭibu-/ṭubu-*		GZ **ṭp-il-* (Kl. 192)
93b. warm					*tebdi*	< Osset. *tävdä* id. (KX 348)
94a. water	*c̣q̇ali*	*c̣q̇ali*	*c̣q̇ari*	*c̣ḳa(r)i*		GZ **c̣q̇a-* (Kl. 304)
94b. water					*lic*	(KX 192)
95a. we	*čven*	*čuen*	*čki*	*čkun/čkin*	(*ni-šgwej* ex. *gu-šgwej* in. our)	K **čwen* (Kl. 256)
95b. we					*näj* ex.	(KX 379)
96a. what	(*romeli* which)		*mu-*	*mu-*	*mäj*	K **ma-* (Kl. 112)
96b. what	*ra*	*ra*				
97a. white	*tetri*	*tetri*			*twet(w)ne*	K **twet-* (KX 303-04); FS 158 adds G *m-tov-are* moon
97b. white			*če*	*(k)če, xče*		GZ *mqc*$_1$*e* (KX 304)
98a. who	*vin*	*vin*	*mi(n)*	*min*		GZ **win* (Kl. 53)
98b. who					*jär*	
99a. woman	*kali*					(KX 37)
99b. woman	(*asuli* daughter)	(*asuli* id.)	*osuri*	(*osuri* girl, daughter)	(*asuš* daughter)	K **asul-* daughter (Kl. 4; KX 37)
99c. woman				*oxorʒa,-ǯa*		L *oxori* house (KX 37)
99d. woman			(*zura* coward)	(*zura* female)	*zurāl*	K **zura-* (Kl. 61; KX 37); cf. Chechen *ʒuda"*, Lak *žura* bitch
100a yellow	*q̇viteli*	*q̇viteli*	*q̇vinteli* < G		*q̇vitel* < G	(KX 315)
100b. yellow				*sari*		< Turk. *sarı* (KX 315)

Abbreviations: Arm. Armenian, ex. exclusive, G Georgian, IE Indo-European, in. inclusive, K Kartvelian, L Laz, M Megrelian, Osset. Ossetic, S Svan, Turk. Turkish, Z Zan (= Megrelian & Laz).

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A Window on the Genetics of Human Speech: The *FOXP2* Gene

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Abstract: The development of human speech seems to be a species-specific and genetically determined capacity and is considered an extremely important step in the rise of modern humans, human culture and civilisation. The multidisciplinary efforts of psychiatrists, linguists and human geneticists led to the identification of genetic elements in cohorts of patients, performing speech and language disorders. A form of special language impairment (SLI) has been identified in the KE family in Britain, as a dominant, autosomal trait, affecting the family members in three generations. Molecular genetic studies revealed a mutation in the *FOXP2* gene as possible basis of SLI in these patients. The unique, human variant of *FOXP2* is shared with Neandertals, indicating a common, ancestral population 3-400,000 years ago. Imprecise imitation of the tutor's song occurs in young canaries with lowered *FoxP2* expression.

Introduction

The spectacular development of molecular genetics has basically changed our view about living organisms. One of these aspects is the surprising fact that the species (fungi, plants, animals) harbour a very similar set of genes either structurally or functionally. Genes, basic regulatory circuits and systems, though developed hundreds of million years ago, are obviously similar in all species studied so far. This observation seems contradictory if we compare with the diversity of living/eradicated species, or even with the diversity of individuals within a non-selected population. This contradiction can be explained by the action of species-specific genes and alleles, determining ontogenesis. Decrease of the evolutionary distances increases the similarity of the genomes, regarding chromosomal numbers and organization, the nucleotide and amino acid sequences of genes and the encoded proteins.

The level of identity in the nucleotide sequence of humans and our nearest living relatives, the chimpanzees is 98.5 % (Ebersberger, 2002). An important difference between us – beyond anatomic, physiologic, social, etc. differences – is that we can talk and they can not. In spite of restless tuition efforts chimpanzees in human environment never acquire the capacity of speech (Terrace et al., 1979). Although the genomics of our nearest extinct relatives, the Neandertals is still in its infancy, the first results demonstrate a very close relationship with modern humans at the DNA sequence level. On the other hand, breakthrough DNA megasequencing approaches enable construction of the first draft of the Neandertal genome in the near future (Green et al., 2006).

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The children affected by SLI develop normally, but at the age of 3-4 years their communication is confined to words, while non-affected children use sentences at this age. Familial cases of SLI and twin studies implicated the involvement of genetic factors at chromosomes 7, 16 and 13. Furthermore, association of language impairment in a broad sense has been established to loci on chromosomes 1, 2, 3, 6, 15, 18, 19 and 21 (Lewis et al., 2006). Association of SLI with mutant alleles of the *FOXP2* gene demonstrated an apparent genetic basis of human speech.

In this paper we summarize the linguistic, psychiatric and genetic consequences of *FOXP2* gene mutations and their impact on human speech.

The involvement of genetic factors in speech disorders

A four-year-old child usually employs a vocabulary of thousands of words and creates complex sentences. In spite of stimulating and communicative environment there are always children suffering from communication deficits, although obvious mental or physiological reasons are missing. A recent survey reported that SLI affects 7% of the population in the USA at the age of six (Bishop, 2001).

The language disability and diagnosis of its deficits involves more or less arbitrary threshold values. The International Statistical Classification of Diseases and Related Health Problems 10th Revision (ICD-10) is a coding of diseases and signs, symptoms, abnormal findings, complaints, social circumstances and external causes of injury or diseases, as classified by the World Health Organization (WHO). According to the ICD-10 guidelines a decline of 2-SD that normally holds for the chronologic age is required for a positive diagnosis.

The diagnostic system offered by the Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition, American Psychiatric Association (DSM-IV, APA, 1994) includes three categories of language impairment: The phonological disorder, the expressive disorder and the combination of both. Whether these subtypes represent separate symptoms remains a question unsolved yet (Bishop et el., 1995).

To overcome the difficulties, rising during diagnosis/phenotype determination in an affected, but unrelated population, twin studies were performed. Monozygotic twins (MZT) are considered genetically equal, whereas dizygotic twins (DZT) share about 50 % of their genes as normal siblings do. MZT pairs diagnosed by criteria for specific speech or language impairment on a broader sense performed a concordance nearly 100 %, whereas DZT pairs showed a concordance rate approximately 50 %, and there was a similarity in the type of disorder in concordant twins also (Bishop et el., 1995).

These results supported the observations reporting accumulation of speech disorders within families. Familial clustering of the disorders argues for genetic risk factors, but these observations may be compromised by the different environments of the family members (age, social background, communicative stimulations, etc.) in nuclear families, compared to the extended family (Lahey and Edwards, 1995). A further support for the involvement of genetic elements in speech disorders rather then environmental factors was provided in a study among adopted children: Speech impairment of the biological parents put their children in a high risk category to develop similar symptoms. This assumption holds for adopted, affected children living with unaffected adoptive parents. On the other hand, no increase of the risk was observed for unaffected children who had adoptive parents with speech impairment (Felsenfeld and Plomin, 1997). The data accumulated in the above family, twin and adopted children studies have furnished hint and encouragement for human geneticists for dissecting the genetic bases of speech disorders. Theoretically, two approaches can be utilized: The assessment of the candidate gene(s) based on the phenotype, and the positional cloning.

Candidate gene approach

There are sometimes pathologic events that allow insights into the protein function, encoded by a mutant gene. A recent paper describes mouse mutants for the a1 and a2 chains of type IV collagen, a major component of the basement membrane, a ubiquitous extracellular element, harboured by all multicellular animals, including humans. The authors suggest that the spontaneous intraorbital hemorrhages observed in the mouse are a clinically relevant phenotype with a relatively high predictive value to identify carriers of *COL4A1* or *COL4A2* mutations among humans (Favor et al., 2007). Indeed, different mouse mutants for the same gene manifest – among others – ophthalmologic phenotypes that may probably have diagnostic power for humans carrying similar mutations (Gould et al., 2005; Gould et al., 2006). Although the candidate gene approach is promising in terms of less labour invested, it can often lead to dead end, as our informations on gene and protein functions are limited.

Positional cloning approach

The vast majority of the human genome consists of non-coding, often repetitive elements. (A recent assessment puts the ratio of coding sequences of the human genome less, than 2%). The family of variable number of tandem repeats includes the microsatellite sequences in humans that consist of a simple DNA sequence (say, the CAT trinucleotide), repeated at variable extent. These microsatellite markers are components of the chromosomes, they are inherited in a Mendelian fashion, exactly as genes do, and are characteristic for each individual. Determination of the microsatellite pattern is therefore suitable for biological identification of human individuals that is widely applied in criminality and paternity tests, or in so called genetic fingerprinting.

Nowadays, positional cloning of a suspected disease gene involves cooperation of clinicians and geneticists, where clinicians establish a cohort of patients suffering in an inherited disease by strict diagnostic criteria, whereas geneticists analyze their genome by establishing linkage map of microsatellite markers, possibly within the same kinship of sanguineous relatives. As microsatellite markers at the same position of the different, individual chromosomes show different morphologies, or lengths in individuals, the probability of each morphologic variants can be determined in the average human population. If the genetic analysis reveals that the affected patients of the same kinship carry the same morphologic marker at certain position of their genome, it can be considered as linkage disequilibrium, i.e. the microsatellite marker may be at a close vicinity of the "disease gene". This observation allows then confining the search on the locus around the disequilibrium marker by involvement of further microsatellite sequences. Usually, the disease locus can be narrowed down to about one million base pairs that may consist of just a few genes and the site of the mutation can be determined by conventional mutational analysis of the coding regions, which normally means sequencing of about 20000 nucleotides.

The symptoms of SLI in affected members of the KE family

Hurst and coworkers described the large, three-generation KE family in Britain, consisting of 37 family members. 15 members of the family, 8 women and 7 men suffered in a severe speech disorder (Hurst et al., 1990). This spectacular way of transmission of the trait revealed that a mutation occurred in a single gene in the family, the gene was autosomal and not X-chromosomal, since roughly equal women and men were affected, and the mutation was dominant, as roughly the half of the family members showed language impairment.

This observation, established by means of classical Mendelian genetics, strongly suggested that even a single genetic variant could disrupt the capacity of speech and contributed to the hypothesis that oral communication was an innate human property (Pinker, 1994). To our knowledge, this is the only case demonstrating the deleterious effect of a single gene on human speech. Although the inheritance of the trait is simple autosomal dominant, the phenotype triggered by the mutation is rather complex. The pathology of the affected individuals arises from the central nervous system (CNS) that impairs several aspects of brain function. The phenotype includes articulatory problems, language impairment and cognitive deficits (Fisher et al., 2003; Fisher, 2005).

Affected individuals have difficulty in controlling the coordinated mouth movements needed for articulated, correct speech, referred to as motoric speech disorder or developmental verbal dyspraxia (Hurst et al., 1990; Vargha-Khadem et al., 1995), but they can execute slow oral movements correctly. The impairment remains in the adulthood even following speech therapy and is not a consequence of the failure of the facial musculature as these patients perform normally in all tests of their limbs (Watkins et al., 2002).

The language impairment extends on both oral and written communications (Vargha-Khadem et al., 1995; Watkins et al., 2002). Affected family members perform significantly worse in written tests of verbal fluency and in spelling unfamiliar words or nonwords (sublexical processes) than the unaffected persons. The phenotype includes deficits in receptive domain, measured by tests of lexical decision, where they were not able to decide whether the presented word is real or nonword in English. Finally, the disorder disrupts both comprehension and grammar production, they have serious difficulties in understanding complex sentences, generating word inflections or word derivation.

The symptoms of the affected KE family members include cognitive deficits also. Their mean nonverbal IQ is significantly lower, than in unaffecteds (Vargha-Khadem et al., 1995; Watkins et al., 2002). This observation raised the hypothesis that the deficit is a consequence of a general cognitive impairment. However, a moderate reduction in nonverbal cognition can be observed in unaffected family members that may mean that this deficit does not necessarily follow the dominant inheritance of the trait, as these persons do not perform speech and language impairments. The other way around, some affected individuals, who have severe speech difficulties, perform normally in nonverbal cognition tests (Vargha-Khadem et al., 1995; Watkins et al., 2002). Inspection of subtests of IQ measurements indicated that the affected individuals had a deficit compared to unaffecteds in learning arbitrary associations between symbols and digits (Watkins et al., 2002). The complexity of the KE phenotype left a question open about the core deficit; Watkins and coworkers (2002) suggest that the performance on a nonword, complex articulated repetition task may be considered the biological marker of the phenotype. This deficit includes impairment in sequencing or procedural learning.

Molecular genetic deciphering the phenotype: The *SPEECH1* gene

Although the debates continued about the core KE deficit, a consensus raised following the above studies that a single, autosomal dominant gene mutation is suitable to disrupt articulated speech and to trigger severe language disability. Taking the advantages of positional cloning, Fisher and coworkers (1998) set up a genome-wide search in the KE family and identified a region on chromosome 7 showing the properties of linkage disequilibrium, a locus in the affected individuals that inherited unchanged. The affected individuals carried the same microsatellite markers in this chromosomal segment, suggesting a common core genetic element of about six million base pairs at 7q31 (chromosome 7, longer arm, band 31). Further narrowing of the map was not possible at that time, since microsatellite markers were not available in such a great variability and number as today. The human genome consists of three billion base pairs; therefore the identified locus represents about two thousandths of the whole genome, but still is a huge chromosomal segment, which may contain 50-100 genes, including the mutant allele of the desired but unknown *SPEECH1* gene, observed in the KE family. Discovery of further microsatellite markers in the 7q31 region allowed narrowing the locus of *SPEECH1* gene up to about three millions of base pairs (Lai et al., 2000).

This achievement itself did not solve the enigma of the exact position of *SPEECH1* gene; fortunately, Lai and coworkers (2000) studied an unrelated patient, CS, who was diagnosed with developmental verbal dyspraxia and language impairment. Karyotyping of CS demonstrated a chromosomal mutation, a reciprocal exchange, or translocation of genetic material between the long arms of chromosomes 7 and 5. This translocation was not observed in the patient's parents and there was no family history of any speech and language impairment. These data suggest that the translocation in patient CS may have occurred during early embryonic development. The observed symptoms and the cytogenetic data suggested the involvement of the *SPEECH1* region in the breakpoint on chromosome 7. The subsequent molecular genetic analysis explored the exact position of the breakpoint within a yet uncharacterized gene, which became literally disrupted. Normally this gene codes for a transcription factor protein, a polypeptide that regulates the transcription of other genes, for example according to the needs of the developing embryo, and provides a spatially-temporally regulated expression of the target genes. Transcription factors are numerous structurally-functionally; the compromised allele in patient CS encodes a protein called forkhead box, family P, member 2 (FOXP2). The spatial structure of this protein family resembles a forkhead or the opened wings of a bird; these domains of the protein interact with the regulatory regions of the target genes, represented by short, special nucleotide sequences, and enhance their expression.

A gene mutation of *FOXP2* in the KE family

All data suggested that the underlying mutation occurred in the *FOXP2* gene of KE family members. Sequencing of the gene revealed a guanine to adenine nucleotide transition in all affected members that caused an arginine to histidine amino acid substitution at the protein level. The arginine exchanged in the affected persons of the KE family can be found in all members of the FOX protein family in one of the "wings" of the polypeptide. This arginine occurs in FOX proteins of all organisms studied so far, in baker yeast *Saccharomyces cerevisiae*, in the worm *Caenorhabditis elegans*, in the fruit fly *Drosophila melanogaster*, up to humans. A mutation of this residue in the FoxN1 protein of the mouse triggers a severe developmental disorder, an immunodeficient nude phenotype, owing to loss of function of the mutated gene product (Schlake et al., 2000). Patient CS and affected KE family members carry one disrupted *FOXP2* allele and harbour an intact copy too. The basis of the onset of the language impairment is therefore haploinsufficiency, the desired protein is present only at a 50 % concentration and can not fulfill its function in critical steps during development of the foetal brain, where *FOXP2* is expressing at a high level among other organs (intestine, lungs) (Lai et al., 2001; Bruce and Margolis 2002).

Animal models for *FOXP2* mutations

Mice are genetically tractable and a mouse model of the orthologous *FoxP2* gene has recently been developed by the gene knock-out technique. This multistep approach results in targeted gene disruption and generates loss of function or null alleles. Mice, like humans, are diploid organisms, and harbour two copies of homologous autosomes inherited from their mum and dad that usually carry the same genes in the same order. Directed crossings of transgenic animals enables generating -/+ heterozygotes, -/- homozygotes and their phenotype can be compared to +/+ homozygotes or wild-type animals. Disruption of both copies of the *Foxp2* gene in -/- homozygotes caused severe motor impairment; the newborn pups died prematurely, and demonstrated an absence of ultrasonic social vocalizations that are elicited when pups are removed from their mothers. Disruption of a single copy of the gene in -/+ heterozygotes led to modest developmental delay but to a significant alteration in ultrasonic vocalization in response to separation from the mother (Shu et al., 2005).

The spatiotemporal expression of the *FOXP2* gene among humans and songbirds is very similar, and confines to the analogous anatomical structures. Therefore, learning of human speech and tutored vocalization of songbirds are comparable behavioral and neural events. During song learning from adult individuals the expression of the *Foxp2* gene increases in the basal ganglia song nucleus, called Area X in young zebra finches. Local inhibition of *FoxP2* expression in Area X of young canaries compromises their ability to incorporate most new syllables of the seasonally changing song (Haesler, 2007). The phenotypes observed in mice and zebra finches strongly resembles the overlapping CS and KE phenotype.

Slightly altered genetic variants of FOXP2 in mammals

The FOXP2 protein is extremely conserved among different mammalian species. The chimpanzee, pygmy chimp, gorilla and rhesus macaque FOXP2-s are identical compared to each other, display one amino acid difference from the mouse, and two differences from the human protein. Orang-utan has two differences from mouse and three from humans (Enard et al., 2002; Zhang et al., 2002). Therefore, only humans among living organisms harbour a protein that shows two different amino acid substitutions: Threonine to asparagine at position 303 and asparagine to serine at 325. The human *FOXP2* gene was sequenced in 54 individuals from all of the continents and these substitutions remained unchanged, indicating that these amino acid alterations in the FOXP2 protein should have been fixed in humans (Enard et al., 2002; Zhang et al., 2002). The study was extended into 29 nonhuman species including one bird and 28 placental organisms. The majority of these species carry a FOXP2 variant with the animal-specific threonine-asparagine substitution with an exception of Carnivora (cat, dog, wolf, wolverine, bear, fox, seal, sea lion), which have a threonine-serine substitution at positions 303 and 325, respectively. This observation suggests that a single human-like substitution is insufficient for acquiring speech and language (Zhang et al., 2002). The time of the onset of these mutations/changes in the human *FOXP2* gene was estimated to occur 200.000 years ago (Enard et al., 2002), or not earlier than 5000 generations or 100.000 years ago. Both estimations are concomitant with the proposed rise of anatomically modern humans about 150.000 years ago. The appearance of the language based oral, and then written communication provided humans with an enormously important selection advantage that lead to the emerging of the early human civilizations as early as ~5000-10,000 years ago.

These estimations must be revised in the light of recent results of Neandertal genomics. Well preserved skeletal parts of Neandertals were discovered at the El Sidrón cave site (Asturias, Spain) with the average calibrated age of about 43000 years (Rosas et al., 2006). Inspection of the *FOXP2* gene of our closest extinct relatives revealed that they shared our *FOXP2* variant. This observation puts the genetic change leading to the present human *FOXP2* gene 300.000-400.000 years ago in the common ancestor of modern humans and Neandertals (Krause et al., 2007), but does not answer the question whether acquiring human *FOXP2* genetic variant itself is sufficient for articulated speech.

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MOTHER TONGUE
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Human Y-chromosome Phylogeography

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Historical linguists and historical geneticists who study the haploid uni-parentally inherited mtDNA and Y chromosome molecules both share the common denominator of phylogeny. Just as languages evolve and diverge, so do haploid DNA molecules as they are transmitted across the generations and persist in the gene pool.

Molecular geneticists can reliably distinguish between ancestral and derived forms of DNA sequence variants by comparison to Great Ape (e.g. chimpanzee) orthologous DNA sequences. Such haploid molecules are immune to the scrambling effects of recombination typical of the autosomal constitution of the human genome and thus are transmitted intact from parent to child. The Y chromosome is paternally transmitted. Occasionally a harmless DNA mutation occurs (typically a single nucleotide replacement such as a C to T nucleotide base substitution) during spermatogenesis and the male offspring not only inherits all the mutations that have accumulated sequentially across the paternal generations but also records new episodes of genetic divergence. The very low mutation rate of such Y chromosome nucleotide substitutions implies that men who shared the derived nucleotide character or allele all descend from a common ancestor. The nested bifurcating patterns of these haploid molecules can be represented as a gene phylogeny.

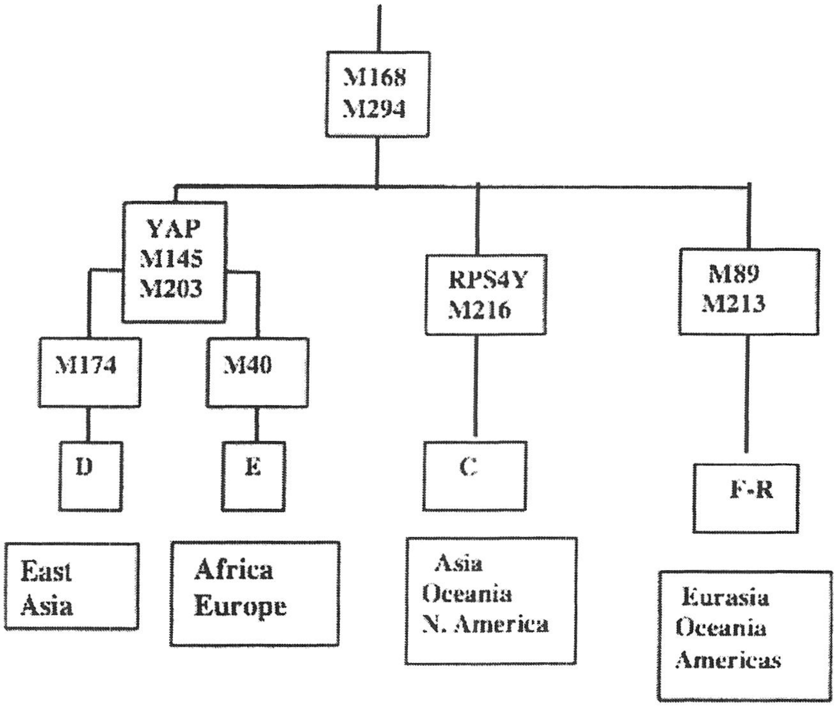
While the stable Y chromosome phylogeny is still emerging as additional DNA sequence variants or markers are discovered, the basic global framework of this gene tree is now defined.

This brief synopsis aims to introduce to linguists how population geneticists attempt to recover the genetic memory recorded as geographic patterns of DNA sequence variation in extant human Y-chromosomes. This evidence provides insights into the origins of Y chromosome molecules and by inference, the origins and substructure of populations.

The genetic diversity of populations represents a combination of both autochthonous molecular innovations and external inputs (*i.e.* gene flows). Thus the temporal and geographic patterns in Y chromosome diversification must be interpreted in the light of the opposing factors of migration and local molecular novelty that occur following population fractionation and isolation. Such genetic drift is accentuated during isolation leading to the formation of distinguishing markers useful for group membership assignment while migration creates geographic affinity. Phylogeographic analysis attempts to integrate patterns of Y chromosome diversification with geography and assumes a correspondence between the overall distribution of haplogroup varieties and past human movements. The strong geographic signal seen in the Y chromosome data is consistent with this assumption (Underhill *et al.* 2001).

One key feature of biography is vicariance, namely a pattern showing widely disjunctive geographic distributions of closely related taxa or sister clades. It is similar to allopatric separation by any casual event. One such example is the ancient heritage of markers YAP, M145 and M203 whose derived allele lie at the root of haplogroups E-M40 in Africa and D-M174 in East Asia (Figure 1). Interestingly, the enigmatic Andamans of the Indian Ocean are haplogroup D and may reflect the descendants of some of the earliest modern humans who successfully dispersed to Asia.

Figure 1



The Y chromosome phylogeny records the entire temporality of anatomically modern human evolution including evidence of its African origin, dispersal(s) from Africa, colonization of the continents, isolation, population growth and contraction, reciprocal migrations and population hybridizations. The current framework of the Y chromosome phylogeny provided the opportunity to expose the general view of global human migration and population origins (*e.g.* Out of Africa). Progress has also been achieved regarding assessments of secondary migrations and colonization events associated with subsequent more localized human evolutionary episodes (e.g. post-glacial range expansions and re-colonizations "after the ice" and demographic induced expansions during the past 10,000 years of the Holocene (*e.g.* the transition of human populations to agricultural economies).

The hierarchical character of the Y-chromosomes phylogeny relates relative temporality to its diversification and progress continues at calibrating the branching events and expansion times of Y chromosomal haplogroups. Since the time of language reconstruction is generally shallower than the genetic record, the bifurcations associated more towards the canopy of the Y-chromosome phylogeny are perhaps of more general relevance to linguists.

Questions amenable to the Y-chromosome phylogenetic approach include: From which geographic areas has a region received its genetic elements? When, how long ago and in what sequence have these connections existed? Are there informative sister groups with a monophyletic clade? Do they display distinctive geography? A well resolved phylogeny permits hypothesis testing and phylogeographic results can suggest alternative hypotheses.

Some considerations regarding haplogroups and migrations include the following: Concerning the deeper internal nodes in the haploid phylogenies, the temptation of embracing the "one haplogroup, one migration" model is likely short-sighted given the complexity and repetition of population movements. The exercise becomes more plausible as one evaluates genetic structure near the tree canopy. However distinguishing tips from more interior nodes in the phylogeny is not always obvious especially in the case of a still emerging (i.e. immature) phylogeny like that of the haploid Y-chromosome. More binary marker sub-haplogroup resolution and analysis of faster mutating microsatellite (also called short tandem repeat, STR) loci linked to haplogroup specific collections of Y-chromosome varieties provide pathways forward.

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MOTHER TONGUE
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Baŋgi me:
A language of unknown affiliation in Northern Mali

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TABLES

Table 1. Comparative records of names applied to the Banga-na
Table 2. Banga-na villages with locations

1. Introduction

This is an annotated wordlist of the Baŋgi me language, spoken in northern Mali. The wordlist was collected with the assistance of Kabo Bamani from a group of villagers in Niana on the 2nd and 9th of March 2005[1]. The informants were;

Informants:
Yamba Babaji
Kunja Kasambara
Baba Tarawali
Ali Babbaji
Kola Basogo
Samba Babbaji

all over sixty years. I would like to thank them for their patience as the elicitation sessions were long and sometimes passed through several languages. Their picture appears below.

This language was subsequently worked upon by the late Stefan Elders, a long-term colleague and friend, who regrettably died in Mali following complications from a stomach ailment on 19th February 2007. His notebooks may well contain much more detailed information, but it is unclear whether it can be used without further work. This publication can therefore serve as an interim measure as well as a memorial to Stefan.

2. Information about the language

2.1 Nomenclature

This language has quite a number of alternative names, given the small quantity of published research. These are shown in Table 1;

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[1] The mission was conducted under the auspices of the Mission archéologique et ethnoarchéologique suisse en Afrique de l'Ouest (MAESAO), Genève. I would like to thank the director, Eric Huysecom for support in this work, also my colleague, Denis Douyon of the Université de Bamako, who was part of the broader research on Dogon languages, but was not involved in this particular study. Thanks also to Lee Hochstetler for general advice and arranging access to the original electronic files of the SIL survey.

Table 1. Comparative records of names applied to the Banga-na

Reference	Language name	Comment
Bertho (1953)	Dyeni or Yɛni	Village name
Calame-Griaule (1956)	Bãñgeri mẹ	Endonym
DNAFLA/DRLP (1981)	Numadaw	Unknown
Togo (1984)	Noumandan	Unknown
Plungian & Tembine (1994)	Numa-daw	Unknown
Plungian & Tembine (1994)	Elebo	Outsiders' name
Plungian & Tembine (1994)	Bangeri-me	Endonym
Hochstetler et al. (2004)	Baŋgeri-mɛ	Endonym

The terms 'Numadaw' and similar were completely unknown to speakers. This survey found the language name to be Baŋgi-me and the name of the people to be Baŋga-na. The intrusive –ri- is found in many records of endonyms in this area (e.g. Duleri for the neighbouring Dulɔ Dogon and probably derives from Fulfulde.

2.2 Location and settlements

Baŋgi-me is spoken in seven villages east of Karge and reached by turning off the Sevare-Douentza road 38 km. north of Sevare. Table 2 shows the names of these villages recorded by the present survey with grid references as well as the 1987 population, where this was given.

Table 2. Banga-na villages with locations

Official	IPA	pop. 1987	N	W
Bara	Bara	211	14:48:20	3:45:30
Bounou	Bunu	418	14:47:50	3:45:40
Niana	Nyana	241	14:48:10	3:46:50
Die'ni	Jene		14:47:10	3:45:50
Digari	Digarɔ		14:47:40	3:46:50
Doro	Dɔrɔ		14:49:20	3:47:20
Due	?Jeni		14:48:20	3:47:00

Source: survey and Hochstetler et al. (2004)

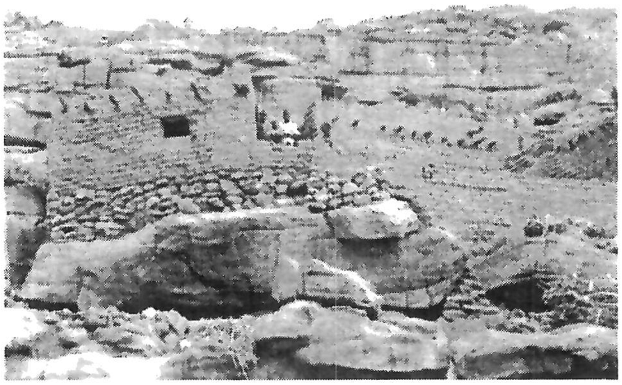
Visual observation does not suggest major increases in size since the 1987 census, but the uncensused villages are at least equal in size to those recorded. The population of Baŋgi-me speakers is likely to be 2-3000.

2.3 Language status

The Baŋgi me language is presently being transmitted to the children. However, there appears to be a loss of complex vocabulary. For example, the numbers above ten have been replaced in ordinary speech and some lexical items were only recalled by elder speakers. The second language of Baŋgi me speakers is Niononkhe, the Mande language spoken in Karge. Niononkhe is a dialect of Bozo or Sorko and is referred to as Sɔgɔ. Fulfulde, a dominant language in the zone, is known to some individuals and there is a limited amount of French spoken, usually by migrant workers or students. These languages are the source of a small number of loanwords. There are no schools in the Baŋga na villages but some children go to the state school in Karge.

2.4 Baŋga-na culture and history

The Baŋga-na are farming people and their distinctive names for crops suggest that they were farmers prior to the expansion of Dogon in their area. Their economy has been transformed in recent years by a move from their mountain villages to the plains and the growth of market gardening. The photograph illustrates the extremely rocky terrain in the Baŋga-na area;



The Baŋga-na are now all Muslims, and this represents a great cultural loss. Possibly aspects of their pre-Muslim culture are recoverable with more in-depth fieldwork.

2.5 The classification of Baŋgi me

All the authors that have written about Baŋgi me have noted how different it is from other Dogon varieties. The only published data on this language is the short wordlist of 'Yɛni' in Bertho (1953:433) which appears to be accurate and the hundred words collected by the Durieux in 1998, cited in Hochstetler et al. (2004). These latter forms incorporate significant elements from the bound morphology and should thus be used with care.

Bertho (1953:413) considered that the affinities of the Dogon languages as a whole were with the 'Voltaic' languages (i.e. Gur) but placed Yɛni in its own group. He says;

> Le dialecte Dyéni ou Yéni des Dogon du canton de Lèol-Géou est le plus aberrant; néanmoins, is se distingue nettement du Bozo-Mandé et du Peul. Il possède d'ailleurs autant de radicaux Voltaïques que les autres dialectes Dogon; mais ces radicaux ne sont pas les mêmes radicaux Voltaïques que ceux conservés par les autres dialectes Dogon, comme si le dialecte Dyéni s'était séparé de l'ancetre Voltaïque soit à une autre epoque que les autres dialectes, soit en un autre point du groupe Voltaïque, lequel, comme on le sait, s'étend de Sikasso au Soudan jusqu'à la frontière de Nigéria.

Unfortunately, Bertho presents no data to justify his argument and no particular relationship with Gur is apparent in the present data. Calame-Griaule (1956:66) says;

> C'est un dialecte unique en son genre parlé dans le canton de Léolguéou-Nonnonké qui représente moins de 1.000 habitants; il est tout à fait aberrant et ne ressemble à aucun autre, bien qu'il se rattache à la langue dogon par sa structure. Les autres villages de la région parlent le bozo.

and again in Calame-Griaule (1968:viii):

> D'un autre point de vue, l'étude du petit dialecte appelé /báŋeri mé/, parlé par une petite fraction de Dogon à l'extrême Nord-Ouest du pays, et qui, bien que reconnu comme «dogon» par les autres, semble présenter des caractères totalement aberrants, serait fort utile pour établir des critères d'appartenance linguistique.

The lexicostatistical table in Hochstetler et al. (2004) records percentages below 10 with other Dogon lects and this would usually be taken to exclude a language from an established grouping. This survey, based on much more extensive material, finds no reason to alter this view, and as a consequence, Baŋgi-me is treated as a language isolate. Indeed, given that it is surrounded by Dogon speakers and has a grammatical structure similar to Dogon, it is remarkable that the percentage of Dogon words is not higher by the usual process of language interaction.

The wordlist given below notes external, Dogon and other cognates where these can be identified. Some of these are tenuous at best. Baŋgi me *does* have some Niger-Congo roots not attested in Dogon, but not enough to establish its membership of the phylum.

3. Phonology

The phonology of Baŋgi me is based on rapid observations and should therefore be regarded as tentative at this stage.

3.1 Vowels

Baŋgi me probably has seven phonemic vowels;

	Front	**Central**	**Back**
Close	i		u
Close-Mid	e		o
Open-Mid	ɛ		ɔ
Open		a	

Baŋgi me permits all vowels to be long or short and all short vowels can be nasalised. Examples of nasalised long vowels are rare, principally the central vowel, e.g.;

cheek	**akããwa**
twenty	**tããwa**

But also;

tree sp.	**kɔ̃ɔ̃ myɛ**

Baŋgi me and other Dogon languages permit sequences of two tone-bearing vowels which contrast with sequences of vowel plus semi-vowel.

bɔ́ù	father
déù	river
dianki	add to
giɛra	rub
kìárá	answer
shiã	strong
shio	sew
sìé	wipe (nose)

These are assumed to derive from intervocalic consonant deletion. In one case this can be demonstrated, as the following forms co-exist synchronically;

fat	**banu ~ bãũ**

It is likely that i + V → yV and V + u →Vw, whereupon the tone is levelled. Some Dogon languages permit more rising and falling tones than Baŋgi me, suggesting this levelling process takes place at different speeds.

3.2 Consonants

Baŋgi me consonants are as follows:

	Bilabial	Alve-olar	Alveopal atal	Palatal	Velar	Labial-velar	**Glottal**
Plosive	p b	t d		c j	k g		
Nasal	m	n		ɲ	N		
Trill		r					
Fricative	ɸ	s z	ʃ		[ɣ]		h
Approximant	ʋ			y		w	
Lateral Approximant		l					

There appears to be certain amount of allophonic or free variation between voiced and voiceless alveolars and velars especially in medials. Thus a single elicitation session two speakers may produce the same word with k~g, t~d. An unusual phoneme is the bilabial fricative /ɸ/;

breast (female)	**ɸyɛ**
chicken	**ɸyɛ**

These are in contrast with;

guinea-fowl	**pyɛ**

supporting the phonemic status of this sound. This phoneme does not occur in neighbouring languages as far as is known.

A voiced velar fricative occurs in some words, such as;

chop down	**dɔɣɔ**

No examples of contrast with /g/ have yet been recorded.

Implosive /ɗ/ was recorded in the phonetic data, as with several Dogon languages in this area, but there is insufficient evidence to set it up as a phoneme, and it may be within the normal range of variation. For example;

village	ɗíyà

Labialisation and palatalisation

Baŋgi me permits contrastive palatalisation and labialisation of some consonants:

Cy

ɸyɛ	breast (female)
gadyɛ̃	world
gyɛmɔ	fence
jyɛnshɛ	chief
kyĩ	canoe
myû	I, me
mbyɛ	arrow
nye	mushroom
nnyɛ	yesterday
pyɛ	guinea-fowl
syɛ	hawk
tyɛrɛ	load
ʋyɛ	water
wuyuwye	fonio

Cw

bwɛ	leg
dwa	tree
gwɔ̃	man
jwɛ̌	dream
kwere	fighting
mwɔ	sore/wound
twoy	narrow
ywɛ	moon

Baŋgi me also allows labialisation with the bilabial approximant for alveolars, which may form contrastive sets with the labial-velar approximant;

tʋɛ	ashes
dʋɛ	clay

Very common in Baŋgi me and unrecorded in other Dogon lects is a labial-palatal prosody. This occurs with many consonants;

cwyã	head-pad
dwyɛmbũ	yellow
gbwyɛ	mosquito
gwyɛ	land/country
kwyɛ	bark
nwyɛ	oil
pwyɛ	wife
twyɛ	termite

Long consonants

Baŋgi me, like the Dogon languages, permits some long consonants. Typically these are nasals;

m	**mmɔntɔri**	patas monkey
	dɔmmɛ	mud
n	**nnafɛ**	west
	nnesaw	why?

Word-medially, these may arise from morpheme boundaries, but there is no evidence for vowel deletion to explain their presence in the initial slot. Although some Dogon language permit tone-bearing nasals in initial position, there is no evidence for this in Baŋgi me.

One case of /yy/ has been recorded, and this also occurs in other Dogon languages.

yya	who?

/ll/ was recorded in some loanwords from Fulfulde.

Nasals and nasalisation

Baŋgi me has homorganic nasals both medially and word-initially. Thus;

mp/mb

mpa	friend
kampaw	pincers
mbyɛ	arrow
tɔmbe	cooling bowl

nd/nk

ndara	to plaster
yindɔ	two
nkwa ati	rain
nwankɛ	leopard

Baŋgi me permits doubled nasals at morpheme junctures. Thus;

minna	door
dɔmmɛ	mud
nnɛ	four

More unusually, it also permits long nasals initially. The nasals are not tone-bearing.

bush	**nna**
patas monkey	**mmɔntɔri**

3.3 Tones

Baŋgi me has two tones, High and Low, and very restricted glide tones. An example of the two-way contrast is as follows;

dɛ́gɛ́	sickness
dɛ̀gɛ̀	head

Rising tones occur on pronouns. For example;

myŭ	I
ăw	you pl.

The tone is contrastive with;

àw	you sg.

4. Morphology

4.1 Morpheme structure

The great majority of Baŋgi me words end in an open syllable. In the syllable-final slot, the semi-vowels /y/ and /w/ are permitted, as well as /ŋ/. The first syllable of

pɛrndɛ	tear

appears to permit /r/ but this may be an assimilation phenomenon.

4.2 Nouns

Plurals of nouns in Baŋgi me are typically formed by the suffixed marker **pɛɛrɛ**. Thus;

nɔɔrɛ	bone	**nɔɔrɛ pɛɛrɛ**	bones

However, these suffixes typically apply to animates and separable objects. Thus ‘head’ and other body parts have no plurals. Baŋgi me has a few irregular plurals;

Gloss	sg.	pl.
ear	**taŋa**	**taŋa-nɛ**
person	**yiwɛrɛ**	**yamba**

4.2 Pronouns

The paradigm of pronouns in isolation is as follows;

mi	I
àw	you sg.
ka	he/she
nnɛ	we
ǎw	you pl.
ni	they

Pronouns incorporate negative clitics (see §5.3).

5. Syntax

5.1 General

Eliciting sample sentences reliably in Baŋgi me is difficult because of a lack of French speakers familiar with sentence paradigms. Speakers constantly tend to re-translate referential sentences to their own point of view, restructuring pronouns and thus verbal forms. Examples in this section must be treated with the greatest of caution.

5.2 Qualification

Baŋgi me numerals follow the noun qualified. Thus;

kurɛ tiri	dog one

The noun does not agree in number when high numerals are applied;

kurɛ yinu	dog two
kurɛ tar	dog three

Lower numerals have slightly different adjectival forms from the count forms.

Ordinals are expressed by the word **ntigɔrɔ** following the expression;

kurɛ dɔrɔ ntigɔrɔ	first dog
kurɛ yindu ntigɔrɔ	second dog

'One' has a different form in this expression, but all other numerals are as in the count form.

Adjectives follow the noun and are invariant;

kurɛ pɔrɛ	dog black

5.3 Constituent order

Like Dogon, the basic word order of Baŋgi me is SOV. Thus;

myŭ	**bòrèfĩ**	**ndyá**
I	food	eat

Unlike Dogon, there appears to be no inflection on the verb in agreement with the pronoun. Thus;

I	**myŭ**	**bòrèfĩ**	**ndyá**
you	**àw**	**bòrèfĩ**	**ndyá**
he/she	**kaw**	**bòrèfĩ**	**ndyá**
we	**nnɛ**	**bòrèfĩ**	**nɛndyá**
you	**ăw**	**bòrèfĩ**	**nɛndyá**
they	**ndɛ**	**bòrèfĩ**	**nɛndyá**

nɛ- is inserted before the verb to mark plural persons.

Negation is achieved via a morpheme –**be** bound to the pronoun. So;

m̀bé	**bòrèfĩ**	**ndyá**
I not	food	eats

I'm not eating food

The paradigm of negative pronouns is as follows;

I	**m̀bé**
you	**àbé**
he/she	**kàbé**
we	**nnɛ̀bé**
you	**ǎbé**
they	**nyìbé**

Baŋgi me operates an aspectual system like Dogon, with a completed/incomplete distinction. The completive verb paradigm is as follows;

Boureima	**dara**	**myǔ**
B.	hit	me

Object pronouns are the same as subject pronouns in the singular. With plural persons the verb is preceded by a nasal. Thus;

Boureima	**ndara**	**nle**
B.	hit	us

and the nasal of the first person plural pronoun becomes a lateral.

Uncompleted verbs are marked by a repetition of part of the verb prior to the object pronoun. Thus;

Boureima	**da**	**mi**	**ndara**
B.	hits	me	hit

6. Baŋgi me wordlist

Abbreviations for loanwords;

B.	***Bambara***
F.	Fulfulde
Fr.	French
S.	Sɔgɔ = Niononkhe
T.	Tamachek

PWS is Proto-Western Sudanic, the quasi-reconstructions of Westermann (1927), marked with # and here standing as a proxy for Niger-Congo.

Wordlists of Dogon lects referred in the commentary can be downloaded at;

http://www.rogerblench.info/Language%20data/Niger-Congo/Dogon/Dogon%20page.htm

where background on each language is also given.

The list includes French as this was the primary language of elicitation.

English	**Français**	**Baŋgi me**	**Commentary**
tree	arbre	dwa	
leaf	feuille	pũyɛ	
root	racine	yi	
branch	branche	kɛmɛ	
bark	écorce	kwyɛ	
thorn	épine	tuŋ	
grass	herbe	gujɛ	
mushroom	champignon	nye	
seed/stone/pip	semence/graine	de	
charcoal	charbon	nyimɛ	
dust	poussière	kure	
ashes	cendres	tʋɛ	
rubbish heap	tas des ordures	diŋi	
mud	boue	dɔmmɛ	
clay	argile	dʋɛ	
dew	rosée	miŋga	cf. widespread #-mi for 'dew' in Niger-Congo
stone	pierre	shimyɛ	
pebble	cailloux	kɔyɔ	
sand	sable	nyimɛ	
smoke	fumée	birɛnyɛ	
fire	feu	birɛ	
water	eau	ʋyɛ	
rain	pluie	zɔŋ	
cloud	nuage	poro	also in Sɔgɔ

English	Français	Baŋgi me	Commentary
lightning	éclair	shirɛn yaga	
rainy season	saison des pluies	nye	
dry season	saison sèche	nyeeru	
year	année	biŋ	
today	aujourd'hui	mwi	
yesterday	hier	nnyɛ	
tomorrow	demain	bɔrɔ	
morning	matin	dinahũ	
evening	soir	kɔmpe	
day	jour	nnɛɛhũ	
night	nuit	yihũ	
moon	lune	ywɛ	Similar forms widespread in Dogon, e.g. Ambaleeŋge **ywɛ**
sun	soleil	nɛ	
star	étoile	toromyɛ	
wind	vent	pɛʋɛrɛ	
sky	ciel	dege	
god	dieu	ŋara	
world	monde	gadyɛ̃	
ground	sol	gwyɛ	
river	fleuve	déù	Resembles widespread Dogon forms, e.g. Bunɔgɛ *déw*, Ampari Pa *diwo*
stream	rivière	ŋgɔɔmbɛ	
pond, lake	marigot	taɣaya	Resembles widespread Dogon forms, e.g. Tebul Ure *taɣa*, Ampari Pa *tay*
hill	colline	shimyɛ	
bush	brousse	nna	
field	champ	bwo	
market	marché	kũ	
house	maison	ko	
room	chambre	yandɛn turumina	
wall	mur	kɔnɔ	
roof	toit	taŋa	also in Sɔgɔ
shelter	abri, hangar	jaŋa	
granary	grenier	paŋgara	Resembles widespread Dogon forms, e.g. Nyambaleeŋge *paŋga*, Walo-Kumbe *pààŋgà*
well	puits	pɔrɛ	
road, path	route, sentier	yɛmbɛ	
village	village	dĩyà	
public place	place	ganda	
human being	personne	yiwɛrɛ	*pl.* yamba
man	homme	gwɔ̃	
woman	femme	nyɛrɛ	? cf. Some Dogon, e.g. Pirõ *yaana*.
child	enfant	yaamɛ	

English	Français	Baŋgi me	Commentary
husband	mari	akande	
wife	épouse	pwyɛ	
father	père	bɔ́ù	Dogon usually has *ba*, but some varieties have a back vowel, e.g. Tomo Kan *bõ*. Miombo *bɔbɔ*
mother	mère	ɲa	Dogon usually has *ni*, but some varieties have a central vowel, e.g. Donno Sɔ *na*, hence this might be proto-Dogon
friend	ami	mpa	
chief	chef	jyɛnshɛ	
hunter	chasseur	shishɔgɔ	
weaver	tisserand	dɛgɛ	
thief	voleur	punsh^{y}ɛ	
doctor	guérisseur	sawre	
witch	sorcière	jɔnshe	
corpse	cadavre	ya	
blacksmith	forgeron	tuŋwa	
hunger	faim	muyɛ	
horn	corne	sira	
tail	queue	tĩ	
egg	œuf	kũ	
wing	aile	siyɔn ko	
feather	plume	kuyu	Dogon typically has *kulɛ*
anthill	termitière/fourmilière	tunkɔ	'termite' is **tu** in languages such as Bunoge, so this may be analysed as 'termite house'
hole, pit	trou	dɔmbo	
hole in tree	trou dans arbre	gɔmbo	cf. Bunoge *kɔmbo* although this is typically 'hole in ground' for other Dogon lects
poison	poison	pwɔsɔ	< Fr.
load	fardeau, charge	tyɛrɛ	
stick	bâton	gùná	
work	travail	wari	Resembles widespread Dogon forms, e.g. Bunoge *wale*
fighting	combat	kwere	
medicine	médicament	gùná	cf. 'stick'. The conjunction of 'medicine', 'stick' and 'tree' is not uncommon in Niger-Congo
money	argent	tɔ̃	cf. Bunoge *tɔndige*
shadow	ombre	sĩ	
thing	chose	kɛ	Jamsay has *kiyɛ*, so this might be a reduction
land/country	pays	gwyɛ	
sickness/disease	maladie	dɛ́gɛ́	Common Dogon is *uru*, but Ampari Pa has *dɛgɛ*
sore/wound	plaie	mwɔ	

English	**Français**	**Baŋgi me**	**Commentary**
scar	cicatrice	mwɔmparɛ	
head	tête	dege	
eye	oeil	shivɛ	
face	visage	tegoro	cf. 'forehead'
cheek	joue	akããwa	
forehead	front	tegoro	cf. 'face'
nose	nez	shumbe	
ear	oreille	taŋa pl. taŋa-nɛ	Not similar to Dogon #sugu- but resembles Niger-Congo #-toN- 'ear'
mouth	bouche	nɔ	resembles Niger-Congo #-nu- 'mouth'
lip	lèvre	nɔ yɛgɛ	
tooth	dent	nasiŋ	
tongue	langue	nanyɛrɛ	
throat	gorge	gɔndo	
neck	cou	kwa	
chin	menton	shɛmu	
shoulder	épaule	ko	? Cf. Ambaleeŋge and other lects *kɔ̀lɔ̀kɔ̀lɔ̀*
armpit	aisselle	kumpa	
arm	bras	ni	Some resemblance to widespread Dogon forms, e.g. Nyambaleeŋge *nwɛ*, Ampari Pa *numa*
hand	main	niŋkuri	
elbow	coude	niŋkubumɛ	
leg	jambe	bwɛ	
foot	pied	bwɛ kumamɛ	
thigh	cuisse	tɔŋgɔrɔ	
knee	genou	bwɛ kugumɛ	
nail	ongle	kwɔmi shɔgɛ	
breast (female)	sein	ɸyɛ	
stomach	ventre	kuri	Some Dogon forms might be shortened versions of this e.g. Ampari Pa *kwe*, Bondum *kulu*
navel	nombril	bɔŋgɔrɔ	Resembles widespread Dogon forms, e.g. Bunoge *bɔŋgalɛ*, Nyambaleeŋge *bɔŋgɔ*
back	dos	gi	
buttocks	fesses	tukuru	
skin	peau	kiŋgɛ	
bone	os	nɔɔrɛ	
rib	côte	sh^{y}ɛ̃	
blood	sang	yii (friction)	
tear	larme	shivɛ uʊyɛ	'eye water'
saliva	salive	nɔnyɛ	
sweat	sueur	ganawa	
urine	urine	sĩ	

English	Français	Baŋgi me	Commentary
hair	cheveux	kuyu	possible resemble to some Dogon forms e.g. Ampari Pa *kurye*.
liver	foie	bimyɛ	
heart	coeur	bikini	
intestines	intestins	kuruwɛ	
lung	poumon	pùjɛ́pùjɛ́	Common Dogon, e.g. Nyambaleeŋge *pùjùpùjù*
body	corps	noonɛ	
meat	viande	ŋooŋ	
animal	animal	yiriʋɛndɛ	
camel	chameau	nyɔŋɔn mɛ̃	names of this form are widespread in the Dogon area
cow	vache	na	cf. common Dogon and also Niger-Congo #-na
bull	taureau	naaŋ gwɔŋ	
goat	chèvre	bĩ	Naŋa has *bɛri*, which might be cognate but this is not common Dogon
sheep	mouton	ŋgamara	
pig	cochon	sɛŋgɛn nyaŋa	
horse	cheval	boo	
donkey	âne	kɔ̀rɔ̀ŋɔ̀	This is a widespread Sahelian lexeme all the way to Ethiopia, ocurring in Niger-Congo, Nilo-Saharan and Afroasiatic.
dog	chien	kurɛ	resembles widespread roots for 'dog' in Indo-European and Afroasiatic
cat	chat	tuwɔ	
elephant	éléphant	taŋan bogo	
buffalo	buffle	sɔŋɔn na	
lion	lion	yara	< Bambara
leopard	tigre/panthère	nwankɛ	
hyena	hyène	turɛ	Pirõ has *tara*, which is concievably cognate
genet	genette	gunu	
jackal	chacal	naaŋ kureme	
porcupine	porc-épic	kwish^{y}ɛ̃	
vervet monkey	singe	karambɛ	Resembles several Dogon languages, e.g. Walo-Kumbe *kɛ́rɛ́m*, Nyambaleeŋge *kale*.
patas monkey	singe rouge	mmɔntɔri	
baboon	babouin	gumbɛ	Resembles several Dogon languages, e.g. Nyambaleeŋge *gumbɛ*
galago	galago	na tɔmɛ	
squirrel	écureuil	girɛ	
hedgehog	hérisson	nyintu shiŋɛ	
aardvark	oryctérope	dɔldɔlmɛ	
dassie	daman des rochers	kuyɛ	
hare	lièvre	girimɛmɛ	

English	**Français**	**Baŋgi me**	**Commentary**
shrew	musaraigne	nyi	
house-bat	chauve-souris	girɛmɛ	
crocodile	crocodile, caïman	gyɛŋgɛ	
chameleon	caméléon	zigi ya gaŋmɛ	
agama lizard	lézard âgama	kimyɛ	
gecko	gecko, salamandre	gɛdɛmyɛ	
monitor lizard	iguane, varan	bã	
toad	crapaud	bumbu	
frog	grenouille	buguru	
tortoise	tortue	kpumyɛ	
snake	serpent	kɛrnɛkɛ	
cobra	cobre	myɛnɛgɛ	
python	python	yɔŋgi	
snake I		mwarɛ	
snake II		don sɔndɔ	
snake III		don kwɔ̃	
snake IV		durumina	
snake V		kɛrɛmɛ	
snake VI		pɔsɛkɛrɛ	
fish	poisson	ŋɔ̃	
bird	oiseau	dɔrɔ	
chicken	poule	ɸyɛ	
cock	coq	ɸyɛ kaŋgɛ	
guinea-fowl	pintade	pyɛ	
vulture	vautour	kondo kun	
hawk	épervier	syɛ	
owl	hibou	gumbi	
bush-fowl	perdrix	shimu ɸyɛ	
scorpion	scorpion	kɔn duŋgoro	
butterfly	papillon	yana sheme	
mosquito	moustique	gbwyɛ	
spider	araignée	taare	
mason-wasp	guêpe-maçonne	gumbara	
bee	abeille	mirɔ	
housefly	mouche	ywuri	
louse	pou	sama	Resembles some Dogon, e.g. Nyambaleeeŋge *ʃɛmɛ*.
termite	termite/fourmi blanche	twyɛ	
flying ant	fourmi volante	wiwɛntumbe	
locust	locuste	dankiyɛ	
mantis		kindi yiyanjɛ	
oil	huile	nwyɛ	Resembles some Dogon e.g. Yanda *ɲi*, Ampari Pa *nu*, in which case this is a Niger-Congo root.
fat	graisse	na nwyɛ	'cow oil'

English	Français	Baŋgi me	Commentary
salt	sel	gyɛŋjɛ	
soup/sauce	sauce	pi	
food	nourriture/*to*	jyɛkɛ	
beer	bière	kwɔnjɛ	Resembles widespread Dogon roots, e.g. Yanda *kɔnzɔ*, Ampari Pa *kɔnje*.
handle	manche d'outil	shyɛ	
sickle	faucille	kɔmɔ	for rice, fonio. cf. Bunoge *kɔɔmɔ* and other Dogon
fruit harvester	hameçon de fruit	doŋgo	cf. Bunoge *dɔŋgo* and other Dogon
firewood knife	couteau de bois de feu	kɔrɔ	cf. Bunoge *kɔrɔgɔ* and other Dogon
harvesting knife	couteau de moisson	kɛrɛkɛmbɛ	cf. Bunoge *alkɛmbɛ* and other Dogon
cutlass	machette		as French
iron	fer	gyɛŋgɛ	
axe big	hache grande	dyewe	
adze	herminette	dɔwɛ	
sowing hoe	semoir	ciŋgɛlmɛ	
hoe I	houe I	damma	
hoe II	houe II	damba	< Bambara
knife	couteau	ba	Widespread Niger-Congo root but not Dogon. Thus
razor	rasoir	shiribe	general Dogon < Fuldulde
anvil	enclume	teye shimyɛ	
hammer	marteau	shimɔrɛ	
pincers	pinces	kampaw	
awl	alène	too	
tweezers	tire l'épine	kamba	
bellows	soufflet	pupa	
cooling bowl	bol d'eau	tɔmbe	
broom	balaie	gyɛ̃rɛ	
bag	sac	yɛmbɛ	cf. some Dogon e.g. Nyambaleeŋge *jembu*
fireplace	foyer	vwɛ	cf. Ampari Pa *vɔ̃*, though this is not the usual Dogon word
shoe	chaussure	kwɔkɛ	
hat	chapeau	bambara	cf. Bunoge *bambula*, Ampari Pa *bampra*
clothing	vêtement	sɔ̃	cf. Ampari Pa *sɔŋ*
necklace	collier	kwaramã	
ring	bague	durumbe	
bracelet	bracelet	gɛŋgɛmɛ	
mortar	mortier	swakɛ	
pestle	pilon	swa	
pot	pot	wure	
pot I	canari I	paɣa bogo	
head-pad	coussinet	cwyã	
basket	panier	te	

English	Français	Baŋgi me	Commentary
winnowing tray	tamis	yerɛ	
sieve	crible	tɛmɛ	
mat	natte	kɛŋgɛrɛ	
needle	aiguille	misina	
spear	lance	pɔnje	
bow	arc	tɔŋgyɛ	? cf. some Dogon e.g. Nyambaleeŋge *tɔ̃ɔ̃* but #ta is a widespread Niger-Congo root for 'bow'
arrow	flèche	mbyɛ	
quiver	carquois	yɛmbɛ	see 'bag'
rope	corde	boyɛ	
stool	tabouret	kun	
door	porte	minna	
door-frame	seuil	kunɔ	
bed	lit	tãwa	
fence	clôture	gyɛmɔ	
ladder	échelle	pɛ̃	
canoe	pirogue	kyĩ	cf. Ampari, Bunɔgɛ *kĩ*. This is likely to be a loanword in all these languages since there are no rivers on their territory.
paddle	pagaie	yiimbɛ	
bee-hive	ruche	mirum paɣa	
Music	**Instruments de musique**		
long cylindrical drum with two heads	tambour	kwɔŋgɛ	long cylindrical drum with two heads
hourglass drum	tambour	kaluŋgo	This is the common savanna *tambour-sablier* used by the Hasua and many other groups under this name
hollow logs	blocs de bois	pɔrɔmpɔ	
flute	flûte traversière	taare	
whistle	sifflet	pir mɛ	
rattle	hochet calebasse	tumba	
transverse horn I	corne	puru mɛ	
iron bell	clochette de fer	lɛŋguru	
Numbers	**Nombres**		
one	un	tiyɛ	cf. Tommo-So and Teŋu Kan *tii*
two	deux	yindɔ	
three	trois	tarɔ	common Niger-Congo and Nilo-Saharan root #-tar
four	quatre	nnɛ	common Niger-Congo and Nilo-Saharan root #-na
five	cinq	nundi	Many Dogon languages have *nundi* which

English	Français	Baŋgi me	Commentary
			may be related
six	six	kere	Dogon forms are always #**kure** etc.
seven	sept	kiye	
eight	huit	saage	cf. Nyambaleeŋge *seege*, but common Dogon is *seero*
nine	neuf	tɛgɛ	? cf. Donno So *tugɔ* and similar
ten	dix	kurɛ	
eleven	onze	kɛkurɛ na kɛtɛrɛ	10 + 1
twelve	douze	kɛkurɛ na yindɔ	10 + 2
twenty	vingt	tããwa	
twenty-one	vingt et un	tããwa na kɛtɛrɛ	
thirty	trente	taana na dyɛkurɛ	
forty	quarante	debe	cf. Nyambaleeŋge *dee*,
fifty	cinquante	debe na dyɛkurɛ	
sixty	soixante	tama shigo	
seventy	soixante-dix	tama shigo na budukurɛ	
eighty	quatre-vingt	yɔrɔ	
ninety	quatre-vingt-dix	yɔrɔ na budukurɛ	
one hundred	cent	yɔrɔ na tããwa	
white	blanc	kishiwã	cf. Nyambaleeŋge *eʃima* (but this is excepotional for Dogon which is usually *tombo*)
black	noir	kipɔɔre	
red	rouge	kubwyɛ	
green	vert	gujakara	
yellow	jaune	dwyɛmbũ	
heavy	lourd	mɛrɛ	
light	léger	bɛmɛ	
large	grand	kanyoro	
small	petit	kiramɛ	
many	beaucoup/nombreux	pɛ̃	
few	peu	kiramɛ	
all	tout	pɛ̃	
thin	mince	biriwi	
wide	large	wɛtɛŋɔ	
narrow	étroit	twoy	
hard	dur	kolo	
soft	doux/tendre	dara	
sweet	doux/sucré	de	also ‘good’
bitter	amer	shimu	
sour	acide	shimu	
deep	profond	yugu	
long	long	bɛndɛ	

English	Français	Baŋgi me	Commentary
short	court	kurumɛ	
good	bon	de, gãu	also ‘sweet’
bad	mauvais	begaũ	‘not good’
dirty	sale	diŋgi	
fat	gros	banu ~ bãũ	
near	proche/près	wɛrɛyɛrwɛ	
far	loin/lointain	dahakɛwɛ	
beautiful	beau	gaũ	
ugly	laid, vilain	kabɛgɔ̃	
hot	chaud	wa	
cold	froid, frais	dimmo	
strong	fort	shiã	
weak	faible	shiã beway	
ripe	mûr	biway	
unripe	cru/vert	kikara	
full	plein	dɛ̃way	
empty	vide	hakɛtɛrɛ	
Verbs	**Verbes**		
accompany	accompagner	nawere	
add to	ajouter	dianki	
announce	annoncer	naniyɛ	
answer	répondre	kìárá	
arrive	arriver	nu	
ask for	demander	siga	
be born	naître	kura	
beat	battre	dárá	
begin	commencer	tììná	
bite	mordre	taŋwa	
blow (mouth)	souffler (la bouche)	piyu	
blow (wind)	souffler (vent)	pɛʋɛrɛ	
borrow	emprunter	kɔɔnimi	
braid	tresser	munu	cf. Nyambaleeŋe *mundu*,
break	casser	pɛndɛ	
breathe	respirer	nyu	
bring	apporter	dugunɔ	
build	construire, bâtir	kuuma	
burn	brûler	daga	
bury	enterrer	mugɔ	
buy	acheter	ywa	
call	appeler	yɛndu	cf. Yanda *yanda*, Walo-Kumbe *nyìnìì*,
carry on back	porter sur le dos	kumbɔrɔ	
carry on head	porter sur la tête	tʋere	
chew	mâcher	tãwa	Not Dogon, but #-ta is a widespread Niger-Congo root for ‘chew’. Cf. Westermann #ta + N *kauen*.

English	Français	Baŋgi me	Commentary
choose	choisir	kɔmyɛ	
chop down	abattre	dɔɣɔ	
chop/slice	trancher	gwyɛndɛ	
close	fermer	shɔgɔ	
comb	se peigner	piindu	? cf. Ampari Pa *biinta*.
come	venir	nu	
cook	cuisiner	dɛnɛ	
count	compter	nyiwa	
cover	couvrir	shɔgɔ	
crawl	ramper	kukalanda	
cry out	crier	pɛ̃	
cut with axe	couper	sere	
cut with knife	couper	dɔgɔ	
dance	danser	tũwa	
desire	désirer	mwìdá	
die	mourir	yaway	
dig	creuser/labourer	kiinu	
divide	diviser	pende	
do	faire	dugolo	
draw water	puiser	ywɛ̃	
dream	rêver	jwɛ̃	
drink	boire	nnyɛ	Common Dogon is **nɔ**, but Toro Tegu has **nɛ**.
drive away	chasser/éloigner/repousser	pɛrɛni	
drop	laisser tomber	ciwɛ	
dry up	sécher	gɔm(ɛ)rɛ	
eat	manger	jiya	*jiɛ* in Sogo, and possibly related to Niger-Congo root #di.
enter	entrer	minde	
extinguish	éteindre	jiŋwe	
fall	tomber	ciwɛ	
fill	remplir	jaŋway	
find	trouver	kara	
finish	terminer	buŋway	
fly (v.)	voler (oiseaux/avion)	pindo	Worldwide #pVr for 'to fly'
fold	plier	kuŋwa	
follow	suivre	kɔɔnpe	
forbid	interdire	bɛntaga	
forget	oublier	tɛmbaki	
gather	réunir	máŋgárá	
give	donner	nando	? cf. Dogul Dom **nda**, though –ndo is likely to be an affix in Bangi me. Westermann PWS #na *geben*.
give birth	accoucher	kikura	

English	**Français**	**Baŋgi me**	**Commentary**
go	aller	were	
go down	descendre	sã	
go out	sortir	ba	
go up	monter	ywe	
greet	saluer	tiya	cf. Nyambaleeŋge *tiya*,
grill	griller	siwa	
grind	broyer	naŋwa	cf. Nyambaleeŋge *namu*,
grow/increase	grandir, croître	puna	
hear	entendre	no	Mombo of Pignari has **nunde**. Widespread Niger-Congo 'ear'
help	aider	bɔɣɔ	
hide	cacher	daanda	
hold	tenir	taɣa	
insult	insulter	tugɛ	
jump	sauter	pindo	also 'fly'
kill	tuer	ywùrá	
know	savoir	shurɛ	
laugh	rire	ma	The –ma element is both Dogon, e.g Yanda *manda*, Ampari Pa *mati* and Niger-Congo, e.g. PWS -mua- (mu-) *lachen*
learn	étudier/apprendre	kitimɔrɔ	also 'try'
lend	prêter	taw	
lick	lécher	dɛ	cf. Dogon forms e.g. Ampari Pa *dega*
lie	mentir	gyɛ	
lie down	être étendu	bunya barnda	
lift onto head	soulever	pɔmyɛ	
light	allumer	daɣa	
like/love	aimer	mma	? cf. Walo-Kumbe *mbáy*,
limp	boiter	sɛgɛna	
listen	écouter	nnɔrɛ	cf. word for 'ear'
look	regarder	shura	
look for	chercher	naŋ kɔmbɔrɔ	
lose	perdre	tewe	
marry	épouser	dɔmyɛ	
measure	mesurer	tɔŋɔ	cf. Nyambaleeŋge *tuŋwa*,
milk	traire	pɔrɔ	
mix	mélanger	swɔ	
open	ouvrir	kyo	
peel	éplucher	mbunda	
pick up	ramasser	kɔmbi	
pierce	percer	tugur	
plaster	crépir	ndara	
play	jouer	saŋa	
pound	piler	sa	
pour	verser	tumbere	? cf. Nyamableeŋge *tuwɔ*

English	Français	Baŋgi me	Commentary
pull	tirer	jimba	cf. Nyamableeŋge *jimbo*
push	pousser	tɛmbi	cf. Ampari Pa *tumbɔ,*
put	poser/placer	pye	
put	mettre	tinde	
rain	pleuvoir	nkwa ati	
receive/accept	recevoir/accepter	taw	
refuse	refuser	nyaaŋu	
remember	se souvenir	miru	
resemble	ressembler	dɔŋlashe	
return	revenir	nuwã	
ride	monter un animal	ywe	
rub	frotter	giɛra	
run	courir	tigirɛ	
say	dire	diga	cf. Jamsay Tegu *tigaara*, Bunoge *taga*.
scratch	gratter	koyɔ	
see	voir	shura	
sell	vendre	tùná	cf. Ampari Pa *tura*
send	envoyer	tùná	cf. Yanda *tɔŋo*, and possibly widespread Niger-Congo root #-tom. E.g. Dagare *tum*, Nupe *tũ*, Common Bantu #*tum-*
sew	coudre	shio	
shake	secouer	maga	
sharpen	aiguiser	giira	
shave	se raser	ka	cf. Ampari Pa *ka*,
shoot	tirer	ta	Common Niger-Congo root, widespread in West Africa, but not apparently in Dogon. cf. PWS *-tà-*. Related to #*-ta* 'bow'.
show	montrer	tɛrɛ	? cf. Nyambaleeŋge *taro*
sing	chanter	nywɛmɛ	
sit down	s'asseoir/être assis	tiri	
sleep	dormir	do	
smell	sentir	nyu	Not Dogon, but Niger-Congo. cf. PWS #-nyu- (+a), e.g. Kuwaa (Kru) *ɲũ*, Eggon (Plateau) *ɲun*.
sneeze	éternuer	oco	cf. English Atchoo!
snore	ronfler	korokoro	May be sound-symbolic, but cf. Nyambaleeŋge *gɔrrɔ*.
soak	faire tremper	miro	
sow	semer	seʋere	
spit	cracher	tuyu	cf. Yanda *too*, but also other Niger-Congo. PWS tú-, Gonja *tùʔ* etc.
stay	rester	barama	
steal	voler	kũre	
stick onto	coller	ndara	also 'plaster'
sting	piquer	tuguru	

English	**Français**	**Baŋgi me**	**Commentary**
stroll	se promener	kɔmɔrɔ	
suck	sucer/téter	mɔnji	
swallow	avaler	mira	widespread root found in both Niger-Congo and Nilo-Saharan for 'swallow', 'throat'. 'neck'
sweep	balayer	gyɛndi	
swell	gonfler	pɛʋɛrɛ	
swim	nager	jìná	
take	prendre	nyaw	cf. Tebul Ure *ɲaan*, but this is exceptional inside Dogon
take off	ôter	bundi	
taste	goûter	namɛbɛ	
teach	enseigner	kara	
tear	déchirer	pɛrndɛ	
tell	raconter	nakɛmbi	
think	penser	miini	cf. Yanda *maana*,
throw	jeter	guyuru	
tie	nouer/attacher	ba	Not Dogon unless Nyambaleeŋge pago is cognate but a widespread Niger-Congo root. cf. PWS #báli *binden*
touch	toucher, palper	daga	
tremble	trembler	maɣa	
try	essayer	kitimɔrɔ	also 'learn'
turn over	se retourner	gɔmiyɛ	
twist	tordre	nwiwa	
undress	se déshabiller	bunda	
unfold	déplier	sannara	
untie	dénouer/détacher	piindu	
urinate	uriner	shĩ	
vomit	vomir	inyɛ ndi	
wait	attendre	dɛngu	
walk	marcher	wɔrɛ	
want/need	vouloir/avoir besoin	mma	
wash s.t. I	laver q.c.	kɛnɛm	
wash s.t. II	laver q.c.	sanam	
wash self	se laver	tura	
weave	tisser	dɛgɛ	
weed	sarcler/arracher les herbes	dyɛ	
whisper	chuchoter	guyɛmpɔrɔ	
whistle	siffler	kɔɔrɔ	
wipe (nose)	se moucher	sìé	
write	écrire	nyɔŋɔndɛ	cf. Ampari Pa *nɔnda*,
yawn	bâiller	ŋawmɛ	
Q words	**Questions**		

English	**Français**	**Baŋgi me**	**Commentary**
where?	où	kote	
when?	quand	nɛnɛ	
how?	comment	numindɔ	
how many?	combien	nii	
why?	pourquoi	nnesaw	
who?	qui	yya	
what?	quoi	nneshi	? cf. Miombo Makori *ŋgɛsiɛ*
Others	**Autres**		
here	ici	ima	cf. Bunɔge *ma*,
there	là	keʋe	
left	à gauche	bara	
right	à droite	siʋe	
north	nord	sajo	
south	sud	ballere	< Fulfulde
east	est	puyɛ	
west	ouest	nnafɛ	
Pronouns			
I, me	je, moi	myû	Dogon has #mi as does other Niger-Congo
you	toi	àw	Dogon usually has a single back vowel, either *o* or *u* and often a glottal stop.
he, she, it	lui, elle	kaw	
we, us	nous	nnɛ	Ampari Pa has *ni*,
you pl.	vous	áw	
they, them	ils	áw	
everyone	tout le monde	karu	

Plants

English	**Français**	**Baŋgi Me**	**Commentary**
Cassava[2]	manioc	bananku	Widespread form in Dogon and other nearby languages and probably related to languages further south, e.g. Twi banku. Cassava is a recent introduction.
Sweet potato[3]	patate douce	kùù	Widespread form in Dogon and other nearby languages, e.g. Nyambaleeŋge *ku*. May be related to Niger-Congo roots for 'yam'. Sweet potato is a recent introduction.
Wild yam	igname de brousse	kùù pɔrɛ	
Wild yam	igname de brousse	jɛkɛ	
Sorghum[4]	grand mil	sinya	not usual Dogon cf. Bunɔgɛ *ʃinja.*

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[2] (Manihot esculenta)
[3] (Ipomoea batatas)

English	**Français**	**Baŋgi Me**	**Commentary**
Bulrush millet[5]	petit mil	damye	
Fonio[6]	fonio	wuyuwye	
Maize*[7]	mais	darama yere	cf. Bunɔgɛ *dilima*.
Rice*[8]	riz	gɔm myɛ	
Cowpea[9]	nyebe	nnyɛ	
Bambara groundnut[10]	pois de terre	tiga yɛ kurumyɛ	
Groundnut[11]	arachide	tiga yɛ bɛndɛ	
Tiger-nut[12]	souche à manger	muwɔ myɛ	
Nutgrass[13]	souche à parfum	muwɔ shiro	
Garden egg[14]	aubergine	taŋ koro	
Okra[15]	gombo	wa myɛ	as in Sɔgɔ
Birdseye chili*[16]	piment	dandi	< F., Sɔgɔ
Onion	oignon	yagu	
Garlic[17]	ail	tumi	< T.
Tomato[18]	tomate	tamati	< Fr.
Melon (other)[19]	courge	tɔŋgɔrɔ	
Kenaf[20]	kenaf	shɔrɛ	
Sesame seeds[21]	sesam	para	cf. some Dogon, e.g. Ana *palle*,
Gourd (Generic)	calebasse	kwyɛ	? cf. some Dogon e.g. Ampari Pa *koye*
Gourd-bottle[22]	bouteille	tumba	cf. Nyambaleeŋge *tumbe*
warty gourd	calebasse à furoncles	kurundundu	
gourd spoon	louche	kɛŋkɛm mpa	

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[4] (Sorghum bicolor)
[5] (Pennisetum spp.)
[6] (Digitaria exilis)
[7] (Zea mays)
[8] (Oryza sativa/ glaberrima)
[9] (Vigna unguiculata)
[10] (Vigna subterranea)
[11] (Arachis hypogaea)
[12] (Cyperus esculentus)
[13] (Cyperus rotundus)
[14] (Solanum melongena)
[15] (Abelmoschus esculentus)
[16] (Capsicum annuum)
[17] (Allium sativum)
[18] (Lycopersicon esculentum)
[19] (*Cucumis* spp)
[20] (Hibiscus cannabinus)
[21] (Sesamum indicum)
[22] (Lagenaria siceraria)

English	Français	Baŋgi Me	Commentary
Other gourds	autres calebasses	tuŋge	used to water gardens
Cotton	coton	nogu	
Fan-palm[23]	rônier	tĩ	
Baobab[24]	arbre de pain	bore	
Shea tree[25]	karité	wɔrɔ	
Locust tree[26]	néré	jwɛ	
Acacia albida	balanzan	kiyɛʋɛ	
Tamarind[27]	tamarinier	ŋga	
tree sp.	arbre	tuŋ	F. ede
tree sp.	arbre	mu	F. gumeji
tree sp.	arbre	kɔ̃ɔ̃ myɛ	F. nyelbe
tree sp.	arbre	dunju	
tree sp.	arbre	pɔrɔwɔ	F. peguje
tree sp.	arbre	gbɔnɔ myɛ	F. bantineje
tree sp.	arbre	kɔ̀ŋgɔ̀rɔ́	F. saman poɗi
tree sp.	arbre	kwa	F. duneeri
tree sp.	arbre	kwĩ	leaves used for soup
liana sp.	arbre	ka	F. poguje

7. Conclusion: the classification of Baŋgi Me

The wordlist gives a substantial lexical sample of Bangi Me and it is reasonable to assume that it is adequate to classify the language. It has been cross-checked against a wide sample of neighbouring Dogon languages as well as synoptic lists of other nearby families such as Dogon, Gur and Kwa. The following conclusions can be drawn;

1. Borrowings from nearby languages, such as Fulfulde, Bambara, Sɔgɔ [=Niononkhe] are relatively few in number. Lexical data on neighbouring Fulfulde lects is poor and the –de ending on some words may point to a few more unidentified loans.
2. Despite the early observation of Bertho that Bangi Me had affinities with Gur, these appear to be very slight and mostly links to Niger-Congo in general.
3. The possibility that Baŋgi me is some type of 'special' or 'secret' language depending on lexical substitution can be excluded, both by the similarity between lexemes recorded by different researchers at different times and the external cognates with Niger-Congo not reflected in Dogon.
4. Although Baŋgi me has a number of cognates with Dogon, these are few in relation to the overall lexical database and are generally not in the core lexicon (body parts, numbers, basic verbs). More

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[23] (Borassus aethiopum)
[24] (Adansonia digitata)
[25] (Vitellaria paradoxa)
[26] (Parkia biglobosa)
[27] (Tamarindus indica)

interestingly, where there are cognates, they tend to be in languages in close geographical proximity such as Bunɔgɛ and Nyambaleenge. This rather suggests that there were once more languages related to Baŋgi me and that its vocabulary exists as a substrate as these populations were Dogonised.

5. In general, African language isolates often look like relic forager populations, even where these have turned to agriculture (e.g. the Laal and Jalaa). The Hadza and Kwadi remain foragers, although encapsulated. However, Baŋgi me has a striking repertoire of agricultural terms quite distinct from the Dogon and neighbouring peoples, which points to a pre-existing farming culture.

From these results it seems likely that the Baŋgi me are the last representatives of a farming population of the Dogon Plateau, assimilated by the Dogon expansion (? ca. 3000 years ago). As such their language and culture are of great importance and clearly deserve intensive study.

As to the affiliation of Baŋgi me, the data suggest two choices; either Baŋgi me is a very early branching of Niger-Congo which has lost a great deal of core vocabulary or it is a language isolate that has interacted with Niger-Congo at an early stage. Its grammar is generally very Dogon-like, and Dogon itself hardly resembles Niger-Congo. Without other related languages, it may be very difficult to resolve this issue. Hausa, for example, is clearly a Chadic language, yet it has apparently borrowed key lexemes from neighbouring Niger-Congo languages, such as the word for 'meat' and 'two' (Hoffmann 1970). This may well be the case with Baŋgi me. It is therefore most probable that Baŋgi me should be added to the select list of African isolates.

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Websites

Roger Blench's website where comparative Dogon wordlists can be found

http://www.rogerblench.info/Language%20data/Niger-Congo/Dogon/Dogon%20page.htm

Jeffrey Heath's website where various lexical and grammatical materials on Dogon can be found

http://www-personal.umich.edu/~jheath/

Site of the MAESAO project for general information on archaeology and prehistory can be found

http://anthro.unige.ch/lap/ounjougou/nouvelles.html

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The language of the Shom Pen: a language isolate in the Nicobar islands

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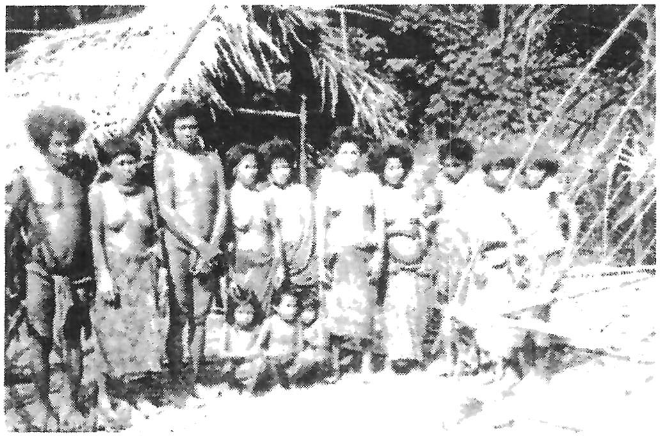
1. Introduction

The Shom Pen are foragers inhabiting the centre of Great Nicobar, the principal island in the Nicobar chain west of Burma and north of Sumatra (Photo 1). The Nicobars were known to Ptolemy and the Arab geographers, but reliable information about them only began to filter out in the 18th century (Singh 2003). Admiral Steen Bille first contacted the Shom Pen in 1846 and De Roëpstorff made the first visit to them to record ethnographic and and linguistic data in 1876 (Man 1886:432).

Photo 1. Shom Pen on Great Nicobar



The identity of the Shom Pen was the subject of much ill-informed speculation in early ethnological texts. Man (1886:429) observed;

> For many years past a belief has been entertained by ethnologists that the inland tribe of the Nicobar Islands (known to the coast people as *"Shom Pen")* would be found to supply the seemingly missing and requisite link connecting the negritos of the Andaman Islands with the Semangs of the Malayan Peninsula.

However, the fact that the Shom Pen generally have straight hair, like the Nicobarese, brought an untimely end to these pleasant speculations. Occasional individuals, as in the above photograph may have wavy hair but it is as yet unclear whether this is genetic or merely unkempt.

For more than a century since Man's original description, only very little has been added to the stock of reliable information on the Shom Pen. Stampe (1966:393) even stated that the Shom Pen were 'possibly extinct'. Rizvi (1990) is a very abbreviated account, while the website http://www.andaman.org/NICOBAR/text.htm provides photographs and some useful information concerning the location of Shom Pen groups in the 1990s. The name 'Shom Pen' itself is Nicobarese. Man (1886:432) asserts that the people call themselves *Shab Daw'a*, although Chattopadhyay and Mukhopadhyay (2003) were unable to find any autonym. According to the Andamans and Nicober website, 'The Shompen have no common name for themselves: those living on the western side of the island call themselves Kalay, those in the eastern part Keyet - with each group calling the other Buavela' and their numbers were estimated at 300 in 2001.

The identity of the Shom Pen has thus remained unresolved. The prohibitions on outside researchers in the Nicobar islands during the from Indian Independence until the present have probably only increased the uncertainty. Although hardly conclusive, given the high incidence of shipwrecks on the Nicobars and the prevalence of incoming strangers, even the issue of the straight hair has been questioned, with some populations apparently having wavy hair. Trivedi et al. (2006) present some genetic information on the Shom Pen, but without reaching any clear conclusions and certainly without substantiating their conclusion that these are 'descendants of Mesolithic hunter–gatherers'. No archaeology of any significance has been conducted on the Nicobars and the time-depth of their settlement is unknown. The 2004 tsunami apparently affected the Shom Pen, but they have by and large survived. As a consequence, the affiliation of their language is clearly a question of some importance.

2. The Shom Pen Language

Until recently, the language of the Shom Pen had remained unknown apart from ca. 100 words recorded by De Roepstorff (1875), the scattered lexical items in Man (1886) and the comparative list in Man (1889). Although our knowledge of Nicobarese is imperfect there are several book-length sources for this group, for example Whitehead (1925), Das (1977) and Radakrishnan (1981). Although most reference books list Shom Pen as part of the Nicobarese group, evidence for this is slight. Apart from some numerals and body parts, the Shom Pen words of show no obvious relationship with other Nicobarese languages or other Mon-Khmer languages. The fragmentary evidence does not immediately suggest that the Shom Pen are Austroasiatic-speakers. Man (1886:436) says; 'of words in ordinary use there are very few in the Shom Pen dialect which bear any resemblance to the equivalents in the language of the coast people'. Man's Shom Pen data shows that numbers 1-5 are roughly cognate with Nicobarese but that above this they are quite different. Man (1886) also observes that there was substantial linguistic variation between Shom Pen settlements;

> In noting down the words for common objects as spoken by these (*dakan-kat*) people I found that in most instances they differed from the equivalent used by the Shorn Pen of Lafal and Ganges Harbour.

A somewhat difficult to access publication, Chattopadhyay & Mukhopadhyay (2003), makes available a significant body of new data on the Shom Pen language. While not to modern standards of presentation and analysis, it is enough to make a more informed estimate of the affiliation of Shom Pen. The authors consider some of the possibilities and conclude that Shom Pen may be related to Polynesian [!]. This paper[1] sets out the extended dataset for Shom Pen and provides whatever etymologies are to hand, using lexical lists such as Grierson (1928) and Shorto (2006) as well as including citations from De Roepstorff (1875). §3. summarises the phonology of Shom Pen, as far as it can be gathered from the authors' presentation. Some of the English glosses are highly local, such as 'dismatting'. I have left these as in the original.

3. Shom Pen phonology

3.1 Vowels

Shom Pen has seven phonemic vowels:

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[1] I would like to thank Laurie Reid for passing on this book, and IIAS, Leiden for making available scanning facilities.

	Front	**Central**	**Back**
Close	i		u
Close-Mid	e		o
Open-Mid	ɛ		ɔ
Open		a	

All vowels have a nasalised counterpart, but only the central vowel /a/ has a lengthened counterpart, ā, which can also be nasalised.

Strangely, Chattopadhyay & Mukhopadhyay (2003) transcribe geminated vowels for the other four cardinal vowels without explaining what makes these distinct from their 'long' central vowel. Most probably Shom Pen has systematic length contrast in vowels. Man (1889) only transcribes five vowels, but his macrons suggest he considered all had a lengthened counterpart.

3.2 Consonants

Shom Pen consonants are as follows:

	Bilabial	**Alveolar**	**Palatal**	**Velar**	**Glottal**
Plosive	p b	t d	c j	k g	ʔ
Aspirate	p^h b^h	t^h		k^h g^h	
Nasal	m	n	ɲ	ŋ	
Fricative	ɸ			x ɣ	h
Lateral Approximant		l			
Approximant	w		y		

Chattopadhyay & Mukhopadhyay (2003) represent many words with final diphthongs such as 'au' and 'ai'. It seems quite likely these are in fact final semi-vowels.

3.3 Orthographic conversions

Difficulties with fonts compelled Chattopadhyay and Mukhopadhyay to use makeshift conventions. The following conversions were made in the lexical list.

4. Annotated Shom Pen wordlist

In the following list, citations from De Roepstorff (1875) are marked R. and from Man (1886, 1889) M.

Table 1. Lexical data on the Shom Pen language

English gloss	PoS	Shom Pen	Commentary
A			
Adam's apple	n.	hõy, ɣiʔuŋāi	
afterwards	adv.	duāg	
algae	n.	komheāu	
ankle	n.	nhiou / ieāo	M. *ang-hē·o*.
annoyed (to be)	v.	hɛhɛn / xeāxi	
ant	n.	ḳolheɔb	
armpit	n.	ginoi	
arum	n.	pugou	
ash	n.	umeõi	
B			
back	n.	gikau	R. *tamnōi*. M. *hok-ō·a*. Some Mon-Khmer languages appear to have a related form. cf. Mượng *khau*. Shorto 1844.
back (side)	n.	coukou	
back (side)	n.	tāi	
back (unraised) side of canoe	n.	guou	
back legs (of animal)	n.	(na) kāieidn	
back of chopper	n.	aŋāine	
back of pig etc.	n.	ekhũā̃u	
bad	a.	phāi	M. *wu-àu·hu*.
bag	n.	ukhuāg	
balancing arms joining a canoe to an outrigger	n.	(na) kānuāi	
bamboo	n.	ŋo / ŋoan	M. *öē*.
banana	n.	omeoin	R. *mum*.
bangle (thick)	n.	konɛŋ	
bangle (thin)	n.	gokuŋāu, koŋkoā	
bark	n.	nho	
bark	v.	kāokāo	
barkcloth	n.	wādou	
bark plate	n.	kũay /kocɛŋ/mhonyāŋ	
basket	n.	teāu	R. *kantjema*.
bat	n.	okināu / kāināi / oiā̃y	
bathe	n.	puoihoɔp / pugoihoɔp	R. *hōhōm*.
bead necklace	n.	luou	

English gloss	**PoS**	**Shom Pen**	**Commentary**
beak	n.	wā̃tā̃m	
beam (horizontal)	n.	gināug	
beat	v.	kuāu / kougāu	R. *hæn-ei.*
bed	n.	xeāub	
bee	n.	uŋāu	R. *holoeñg-wa.*
beehive	n.	komeāu	
belching	n.	geāu	
belly	n.	kāo	R. *kau.* M. *kàu.*
belt	n.	gitui	
bench	n.	koɸeoi	
bend		kāu	
bent (to be)		jeoāin	
betel	n.	nou	
betel nut	n.	nāŋ / nyāŋ	M. *halaig-nâng.*
big	a.	ocuog / imeʔi / kāduoi	
big (as of tree)	a.	niŋāi	
bird	n.	kaiho	R. *sæ-tjo-a.* M. *sichū·a.*
bite	v.	kaidde / hɛkāb	R. *kenjt po.*
bitter	a.	heɸuoi	R. *paka.*
black	a.	kaʔiug	R. *metj.* M. *mēt-kū.*
black hair (head)	a.	ɛuaujuou	
blind	a.	ecoāu	
blood	n.	tiub / teub	R. *tjé-tjéng.* M. *döb.*
blood vessel	n.	xeāin / (ko) xɛ̄̃āi	
blow (air)	v.	nɛphoāi	
blow (with fist)	v.	kouou	
blowing of nasal mucus		bāɸoāg	
blue	a.	komeniaŋ	
blunt	a.	okād	
blunt part of chopper	n.	ukāi	
body	n.	kalhoy	
body ache	n.	xieināu, duoiʔetāi	
body of canoe	n.	māu	
boiling	a.	khoāg	
bone	n.	kāʔeem / kādoā	R. *ka-a-eng.*
bottle	n.	ũā̃u	
boy	n.	kekoāi / cugauʔe	M. *akau, kō·it.*
branch (of tree)	n.	kogāg	
break	v.	huɔŋ / hekāu	
breast	n.	hũigimau	M. *tō·a.* Man's form agrees with common Nicobarese.
breathe	v.	hẽā̃ŋ	
bride	n.	ādiʔou / digeu	
bring	v.	ɣioāy	M. *yau-meṅ.*
broom	n.	kɔleo	R. *mo-i dæt.*
brother	n.	muā	M. *chü·a.*

English gloss	PoS	Shom Pen	Commentary
brother's son	n.	ŋiei	
bulb (electric)	n.	ĩuā̃i	
burn	v.	touoi	
burst	v.	bākuāo	
butterfly	n.	xẽā̃o / okhẽā̃o	
buttock	n.	ŋuināu	
C			
calf muscle	n.	neāu	
camera	n.	ākanyāu	
cane	n.	nāigee	
canoe	n.	toāy / bɛkuāu	R. *kehā*. M. *dō˙ai*, *hö·a*. Man compares this to Nicobarese *dʉ˙e*.
carry	v.	kaiug kao	
carry (with wooden carrier)	v.	nɛtoāu	
carry on back	v.	puggāi	
carry on chest (as child)	v.	naŋ mau	
carry on head	v.	toāgge koi	
carry on shoulder	v.	nɛtoɔŋ	
cat	n.	koceoŋ	R. *tjing*. < Malay *kucing*
catch	v.	laub na tai/laub ku gāo	
catch (s.t. falling)	v.	bāteāu / tha	
cement	n.	cemai	R. *pamain*.
centipede	n.	eab	
chair	n.	ohā̃u	
cheek	n.	neāu / toʔŋeāum	M. *ngē·am*.
chest	n.	gimau	M. *maiṅ-ta-aṅ*.
chest pain	n.	tāimɛthuõ	
chew	v.	heŋāinhe /ŋāiŋom	
chicken	n.	kagai toɸẽo	
child (female)	n.	kagai	M. *akau*.
child (male)	n.	ugābeāu / kagai	
chili	n.	ceu	R. *kóa*.
chin	n.	wāŋāi	R. *mjen te tjean*. M. *kō·id*.
chopper	n.	giu	
clap		thā ki tai	
clean (mouth)	v.	gilāonuām	
clean (with water)	v.	hoɔp	
climb	v.	hecāu	
close (bag etc.)	v.	niŋam	
close (bottle etc.)	v.	hā̃ub	
close (window etc.)	v.	tougāy	
closed		tougāy	
cloth	n.	loe	R. *holōn*. M. *lô˙e*. also

English gloss	**PoS**	**Shom Pen**	**Commentary**
			Nicobarese *lô˙e.*
cloud	n.	kāyāb	R. *gnó-e.*
cock	n.	khekhoāi	
cockroach	n.	peāg	
coconut	n.	leāu /guiāo/ taub/nou	R. *katel, hóa*. M. *kalē·al*. The *nou* term could be related to widespread Austronesian **niuR*.
coconut kernel	n.	jāg / lubiāu	
coconut shell	n.	kāʔeun / bhɛu	R. *hint-jeng.*
cold (s.t. not hot)	a.	kāytāi	
cold (weather)	a.	didɛm	M. *dâm.*
collarbone	n.	hoŋuāg	
collect	v.	muou	
comb	n.	gigai	R. *wén, kutta.*
come	v.	hecāg	
construct	v.	khijāy	
container	n.	doāo / kauŋ	
cook	n.	tii	
corner	n.	ouŋāy	
corner of chopper	n.	kokoi	
corner points of a rectangle	n.	teuŋ	
cough	v.	heiaghe	
cough	v.	ɸeiāghe	R. *o-ong*. M. *ō·ah.*
count	v.	geāide	R. *panne*. M. *yiad.*
cover	v.	toākou	
crab	n.	jiāu / huŋāb	
crawl	v.	teoai	
creeper	n.	niāin	
creeper sp.	n.	nou nɛphoāu	
crocodile	n.	kõuā̃u	M. *âyō.*
crown of head	n.	thāi	
curve (concave)	n.	tĩõu	
curve (convex)	n.	kõiāide	
cut	v.	gehõy	R. *kadenji.*
cut (as of animal)	v.	heiŋāu	
cut (fruit etc.)	v.	hetogaub	
cut (grass etc.)	v.	phoāo	
cut (into small pieces)	v.	tainhõ	? cf. Bahnaric e.g. Stieng *tah* ‘to cut up’. Shorto A191.
cut (leaves etc.)	v.	kugai	
cut (wood etc.)	v.	kagau/ kāitohe/heɸeɔ	
cut hair	v.	hāug ko juou	

English gloss	PoS	Shom Pen	Commentary
D			
dance	v.	geāu	M. *fū·aha*.
dark		cuou	
daughter	n.	meāu	M. *kö·at-apàu*.
daughter's son	n.	teāug	
deaf	a.	okheāŋ	
death	n.	badgam	R. *bá-e* 'dead'.
defecate	v.	hekkao	
descend	v.	heɸuɔg	
dew	n.	komeu	
dig (soil)	v.	ɸeāy	M. *wī·ai(d)*.
dismatting [?]	v.n.	ɣiāi igoki	
dog	n.	kāb	R. *kūp*.
door	n.	hẽāub	
drag	v.	hetogāu	
dragonfly	n.	kohlāi	
draw (water from well)	v.	nacuoi	
draw by hair of head	v.	toākoāi	
drop	v.	kagai nuŋāo / kekoāug	
dry	a.	kuijāg	
dry (fruit)	a.	nuoi	
dry (leaf)	a.	laʔgoeāi	
dumb	a.	muŋou	
dust	n.	lokujāi	
E			
eagle	n.	iuaŋ / taɸeũ	
ear	n.	nāŋ /nyāŋ	R. *gña*. But cf. general Nicobarese *nang*.
earache	n.	tāi ki nāŋ, geāu hi nāŋ	
ear-ring	n.	lāi nāŋ	
earwax	n.	komeu	
east	n.	uhā̃ɸuou	
eat	v.	nuām, bhiou, tāoāgguɸɛg	
egg	n.	tagai to hõ	
eight	num.	thugāy	R. *taw-we*. ? cf. Palaungic e.g. Palaung *tā*. Not in Nicobarese
elbow	n.	wā̃kai jauŋ	
embrace		nɛɸuou	
empty		kheāi	
empty belly		khoāg kāo	
eradicate	v.	kõwā̃in-āi	
estop [?]	v.	ɔue tāu / thā̃kinoāh	

English gloss	**PoS**	**Shom Pen**	**Commentary**
extinguish	v.	neāub / ϕã̄y	
eye	n.	meāin	roots with m- are found throughout the region, Austronesian forms with *mat-*, Tibeto-Burman with *mit-, mik-*, Karenic with *mai-*, Miao *mai.*
eyeball	n.	ŋu meāin	
eyebrow	n.	eou / eiou	
eyelash	n.	(na) bhε̃y	
F			
face	n.	kheigimāŋ	
fall	v.	bεtiāu	
far		tatag	
farn [? fern]	n.	wāϕo	
fast		koi	
fat (as person)		neāu	
father	n.	eem / eam	
father's brother	n.	kokeɔ	
father's father		koai / koāu	
father's mother		kāj	
feather		okhoã̄o	
feed	v.	kaugāi	
fever		theguɔ	
fight	v.	iou	
file (for rubbing)		kaijāb	
finger		(heinuŋ) nuŋāi	
finish	v.	neān	
fire	n.	job	
fireplace	n.	mooijob	
firewood	n.	ŋoug	
fish sp.	n.	kāug	
fish sp.	n.	(ko) leub	
fish sp.	n.	(ko) huŋ āi	
fish sp.	n.	teāb	
fish sp.	n.	kāuāu	
fish-hook	n.	koāin	
fist	n.	laub	
five		tε̃y	R. *tein.*
flame	n.	ϕeāŋ job	
flank	n.	noam	
flat (as of nose)		pẽāb	
flesh	n.	kogāo	
floor of hut	n.	kopheōi	
flower	n.	ϕijou	
fly	v.	kojāi	
fog	n.	koneāu	
fold	v.	niŋam	

English gloss	PoS	Shom Pen	Commentary
fold in front of loincloth	v.	koithoāŋ	
fold of cloth at the waist	v.	toāɸuggāi	
foliage	n.	tātāŋ	
foot	n.	(ko) ceog	
footprint	n.	komeu	
forehead	n.	kumheāŋ /komeāin/bheāu	
forest	n.	gipẽ	
four		phugāi	R. *fu et*. ? P-Austroasiatic **pun?*, **pan*. Shorto 1166.
fracture	n.	mā̃tā̃t	
frog	n.	gõāŋ (non edible) kwā̃o (edible)	
front	n.	comeāin / okāt	
front legs (of animal)	n.	wā̃kāijāuŋ / (nɛ) kugāu	
fruit sp.	n.	kāi	
fruit sp.	n.	touɸiug	
fruit sp.	n.	tiāu	
full		kāduoi / peii	
full belly		nuo e kāo	
G			
get up		ikheāu	
girl		kohā̃t	
give		hetauag / toāgitai	
give (imp)		tāo	
glass		taiag juou	
go		kaiug	
go away		koāug	
goitre		uŋāgā̃i	
good		āuʔe/au/ɛʔau	
grass		khougāu / (ko) khugāu	
grass sp.		kɛb	
grasshopper		okā̃i	
gravel		kheug	
green (colour)		kāʔoõi	
green (fruit etc.)		kõhõoi	
green (leaf)		xineāuŋ	
green (raw)		tameāu	
green coconut		ɸijou/ijeʔā̃dheiā̃i/goāubiou	
greet with folded hands		nuāoktai	
grey hair		taiag juou	
groom		nākāu	
grow		calhoāg	
grumbling		gocuāg	

English gloss	PoS	Shom Pen	Commentary
grumbling (as a boar)		goʔlau	
guava		koneāu	
gulp		gāokheuŋāi	
gum of tooth		iāo-gi-tām	
gum of tree		toāi	
H			
hair (other than head)		okhoā̃o	
hair of head		juou	
half		lahāi/ŋā	
hammer		diāi	
hand		kougāu	
hand (left)		(ko) tiāug	
hand (right)		(ko) kaug /gokou	
handle		nehāg	
hang		hetoag	
hard		cũoi	
head		koi/ɸiāu/hagupeāuŋ	*koi* is standard Nicobarese and a widespread Austroasiatic root.
head of spear		moāi	
headache		tatāuŋ koi/ tɔŋ koi	
heart		meāŋ no hoy	
heavy		cuoid	
heel		wā̃deoceog / deāu	
hem of loincloth hanging behind		wā̃guou	
hen		taɸẽou/ toɸẽo	
here		inhõ	
hiccough		mānheāg	
hide (one self)		keuglāo	
hide (something)		coāu	
high		heleun / cuoid	
his		onā	
hole		kohāŋ	
hole of ear		haguinaŋ	
honey		(nɛ) puŋ ou	
horn		nyɛŋ	
hot (s.t.)		tāi	
hot (weather)		phoai/icigad	
hot sun		tixiug	
husk		kouāu	
husk of wood		khohāu	
hut		hagupẽ / niā̃i	
I			
I		iɔ̃ / ihɔ̃	
incense		(na) tiom	
insect of rice		eidwaŋ ke adoai	
insect sp.		gũiā̃u	

English gloss	PoS	Shom Pen	Commentary
insert (nail etc.)		kougy	
iron		kātooi	
itch		khiu / gikoāi	
J			
jaw		ŋāi, kāʔiuŋ	
jerk		heheu / tɛ̃ou	
joint of bamboo		poāugge	
jump		hẽkõu/ lauuŋ	
K			
kernel (as in guava)		koimuŋ	
kerosene		puoi job	
kick		tauŋhe	
kiss		toɔb	
knee		guāg	
knife		katooi	
knot		pein	
knot		na pein	
kumba (edible plant sp.)		kũā̃i	
L			
ladder (of hut)		igāy	
lame		ẽcag	
land		gihou	
lap		ɣimeāiŋ	
lap of loincloth		kāduoid	
laugh		hāu	
leach		leɔb	
leaf		1ɔi / wā	
leaf of mushroom		bho	
leaf stalk		koāuŋ	
leaf-bud		kooi	
lean		ũoy	
left (side)		hein hā̃i	
leg		ŋuiāuŋ	
leg ornament		pein	
lemon		toāi	
lice		kokoy	
lick		eāu	
lie down		heidigoug/ tounoɸɛg/ ningopoāuŋ	
lie on back		heidigoug	
lie on chest		hehugāb	
lie on side		heteiuŋ	
lift		poeā	
light (in weight)		ākāiɸɛ̃i	
light (torch etc.)		gouduekhɛg	
lime		ɸijou	

English gloss	PoS	Shom Pen	Commentary
lines of palm		ŋhā tha tai	
lip		tõi	
lip (upper)		kotoõi	
listen		gitāgināŋ	
little (a)		ijeʔā	
lizard (child)		kagai kā̃i	
lizard (house)		kā̃i	
long		kocuoug	
loose		ɣināo	
loud		kāduoi	
low		heʔugāo / touāg	
low (tree etc.)		heou	
low (voice)		ekeiɛ̃	
lower side of a leaf		(na) nuou	
M			
make		khijāy	
make fire		tāb	
make fire		tāb	
male		ameā kāu	
man		koleāg / oleāu	
many		guice	
marriage		puggāi	
matchstick		kāicõi	
measure of four fingers		tyāug nuŋāi	
measure of full span		ɸewāi	
measure of one hand		ɸewāi e kugāu	
measure of two hands		ɸewāi e tugɛb	
medicine		tāŋ	
molar tooth		ŋa / niāu	
mole		konāu	
monkey		nihāi / coāi	
moon		houou	
mosquito		opugāi	
mosquito net		juou	
mother		diei	
mother's brother		kokeɔ	
mother's father		koāi	
mother's mother		kāj	
mouth		tameauŋ, ɛguɸɛg/ komeou	
move		hẽin	
much		imeʔi /duoi/ kāduoi/badāi	
mud		hoŋāu	
mushroom		kɔtho	
my		ca	
N			
nail		giob	

English gloss	**PoS**	**Shom Pen**	**Commentary**
nail		kouā̃y	
nasal mucus		hā̃i	
navel		ɸuoij	
near		nɛojeci	
neck		kupeāuŋ	
necklace		luou	
needle		itaub	
new moon		puoŋlāi	
night		cuou	
nine		niŋai	R. *nog-in.*
nipple		mɛthou	
north		uhā̃kāpeɔ	
north wind		(nɛ) tāg	
nose		mhou	R. *monk.* cf. Nicobarese *moah.*
nostril		gumhou	
not		mheāŋ	R. *unāng.*
O			
oil		puoi	
old man		micimāu / kāmināu	
old woman		ādioug / (WO) kāminɛg	
one		heiag	R. *hing.* cf. Nancowry *hēaŋ.*
onion		wā̃ coān	
open		ghuāu	
open (bag etc.)		lāuŋ	R. *wœnde.*
open (bottle etc.)		miou	
open (cloth)		uŋāi kedhɔ̃	
open (door etc.)		ghuāu	
open (eyes)		meaigi, thõ/kātāyat/ghɔ̃y	
open mouth		tameāuŋ	
outrigger		cāo	
outsider		tamiāu	
owl		kaiho ɸuŋāo	
P			
pain		tāi / tɔŋ	
pain in belly		tāi-ike-kāo	
pain in chest		tāi gimāu	
pain in hand		tɛŋkugāu	
pain in leg		geāu tameān/heiugwā̃i gineā	
palm of hand		hagug tha tai/tha tai	
pandanus		hlā/buggai	
papaya		babai	
parrot		kaiho kāiāi	
peel		khẽāi gekoi	
penis		tāub/ tugāo/egoāi/goāi	
pierce		tuŋāo	

English gloss	**PoS**	**Shom Pen**	**Commentary**
piercing of thorn		wā̃dākāi	
pig		(na) lau	R. *noñg*. wild pig *alœv*. perhaps cf. Mon-Khmer. Palaung *le?*, Luang *ləic*.
pigeon		ohā̃um	
piglet		kagai lau	
pile up		muou	
pillar		keuŋ	
pinch		guggāi igoki	
plant		kwāe / kouglāy	
plant sp.		pũā	
plant sp.		geāi	
plant sp.		neug	
plant sp.		meuŋ	
plant sp.		nibbhõi	
plant sp.		kāie	
plant sp.		tei	
plant sp.		nɛkuā	
plant sp.		theāg	
plant sp.		binoi	
plant sp.		nɛkugāo	
plant sp.		nɛtiāu	
plant sp.		huāug	
plant sp. (*cyathea*?)		tāi	
plant sp. (*dillenia*?)		ɸugāu	
plant sp. (*Dinochloa*?)		kagou	
plant sp. (*dinochloa*?)		khougou	
plant sp. (*dinochloa*?)		bāgue	
plant sp. (*gleichenia*?)		guāu	
plant sp. (*tinospora*?)		nɛɸoāu	
plate (of bark)		kũāy	
pluck flower		tāoɸijou	
poke		tauŋ	
pole		togiāi/ koteũ	
pork		tāb	
pot		tyāug/tõã	R. *awœk.*
potato		ālu/ kohuāu	*ālu* < Hindi.
pour		gigou	
prawn		okeɔb	R. *bo-it*.
prepare		khijāy	
press		hẽineitā̃o/leitāŋ	
pull		touay/ tao	R. *gno-en*.
push		ghuou	
put		touāug/ tapeāg	
put off (candle etc.)		neāub	
python		miŋāi	
Q			

English gloss	PoS	Shom Pen	Commentary
quarter		teāŋ	
R			
rain		pĩ ā̃i	
rat		niŋāi	
rectangular		kũiou /uŋāine	
red		kāteob	
remainder of fruit etc. after chewing		egānmɛ	
rest		kācām	
rib		tanuāy	
ribbon		gõiajhe	
rice		aduoi	
ridge of palm		koaŋ tai	
right (side)		anikā	
ripe (fruit etc.)		toāu	
ripe (fruit)		toāu	
road		kauāu / kaiugŋhā̃	
roast		ɔŋān	
roll (thread etc.) in the fingers		uŋain	
roof		nɛteāi	
root		(ko) ghiāu	
rope		nāi	
rough		xeāidā̃o	
round		tĩɛ̃u/nijãg/eiāo	
rub		hejāoginou	
run		ɸeāu ghāu	
S			
salt		kādāb	
salty		kumeoin	
sand		moiunāu/ komeoigo	
sapling		ɸẽā̃u/pũi	
scale of fish		thẽā̃u	
scar		kopeŋāi	
scatter		naphooi	
scorpion		tiāu/kokugāu	
sea		(ko) cuāg-i	
seat of canoe		tigei/dɛo	
see		kāiāy	
seed		koŋuõu	
semicircular pieces of wood fixed on the front and rear of a hut roof of hut		tāi	
seven		ā̃in	R. *oin.*
sew		ghõā̃u	
sexual intercourse		hecɛŋ	

English gloss	PoS	Shom Pen	Commentary
shadow		kau	
shake hand		tāo tai egakayuk	
sharp		kāi	
sharpen		neodām	
shave		ɸeaŋ	
shell		tẽā̃in	
shoe		tɛceog	
shoot		koijaug	
short (as of a tree)		heaou	
short (person)		kāgāu	
shoulder		kokogeu	
shout		ĩā̃o/nhā̃b	
shut (door etc)		goiāu	
shut eyes		meumeāin/pheāg/hā̃ub	
shut off (as of radio)		neāub	
sing		yugāi igoki	R. *ejū.*
singlet (inner garment)		nakāijoi	
sip		eɸoāi	
sister		iā̃	
sit		kākoāy	R. *gñidé.*
six		ugāo	R. *lœv-ve.*
skin		kouāo / kouāu	
skin disease sp.		kāi	
skin disease sp.		kolāu	
skin of coconut		kouāu leāu	
skull		kāʔeem koi / tomeāpeāuŋ	
sky		ginhāu/eiõu	
slap		hāneɸeāg	
sleep		bātāiāg	
slippery		waijaid	
slope of hill	n.	hlāid	
slow		okɔy	
small (in quantity)		uduāi	
small (in size)		kāgujāg	
smell		heiei	
smoke		phoŋāu	
smooth		taijaid	
snail		hõā̃um	
snake	n.	kāiā̃u/nuŋāi/giāo	
snake (green)	n.	leāk	
sneeze		ĩā̃o	
snore		hā̃cuāg/ khẽā̃in	
soft		kāpuāu	
soil		uri~o	
sole of foot		tha ceog	
son		āteāg	

English gloss	**PoS**	**Shom Pen**	**Commentary**
song		yugāi	R. *kó-et.*
son's son		teāuˑg	
sound		teteiu	
sound of sour taste		diāu	
south		uhā̃goāu	
south wind		niāŋ	
spade		pata-u	
spear (with cut marks)		guhāi	
spear (with three or five barbs)	n.	bekugāi	R. *kakang.*
spear type	n.	yugāb	
spear type	n.	bākugou	
spider	n.	juāg /jugɛb	
spider web	n.	komhõin/niāijugɛb	
spine	n.	kāiŋkeāb	
spit		phoāi / dei /heidei	
split		ɸeāy	
spoon for stirring rice	n.	uhā̃u	
sputum	n.	phoāi	
squeeze		uiõu	
squirrel	n.	koāg	
stalk of coconut	n.	nehāg	
stand		naigicām	
star	n.	gekhāŋ	
startling		konheāu	
stay		kācām	
steal		wā̃i igoki	
steam		kotoõi	
stem		neāu / māu	
stem of mushroom		ɛi	
stick		nāidɔŋ	
stick of canoe used as handle		ekhuoi	
sticks connecting the two semicircular planks on the roof of a hut		kāub	
stir (rice etc.)		kuou	
stone		kheu/(nā) ŋoāin	
stool		okaet	
storm		ŋāii	
straight line		tugou	
strain off (water from rice)		gigou	
strangle		kanuoi	
stream		‘cuāg	
strike		ɸeāo	

English gloss	**PoS**	**Shom Pen**	**Commentary**
submerge		touŋ	
suck		bɛtuāu	
sugar		cini	
sun		xiug	
sunlight		xiug	
sun rays		xiug tɛm	
sunrise		kācāug	
sunset		diguoxiug	
swallow		gao xeuŋāo	
sweat		komeu/oɸeoi	
sweep		hoɔp tehā	
sweep (water)		napĩā̃i/ɸiā̃oggoi	
sweet		ɣiɸiu	
swim		ijaug goi	
T			
table		koɸeoi	
tail		guou	
take		hetāo/ tāogheu/ tauag	
tall (person)		hacugāu	
tall (tree etc)		niŋāi	
tasteful		āo-e-bhiu	
tasteless		na poāi	
tear (as of grass)		ghuou	
tear (paper etc.)		iāy	
tears		nheā̃u	
ten		tai	R. *te*. ? cf. Burmic languages which have *ta* or similar.
testicle		ŋuouŋ/gugāug	
theft		wā̃i	
there		tanhāi	
thick		tāu/duoi/getāo	
thick (as thread)		kāduoidāug	
thigh		(hethānā) lau/ gau	
thin		hā̃ub/thaāgge	
thin (as thread)		ijeʔā̃	
this		eihõ	
thorn		teāg	
thread		nāin/ wānāŋ	
three		ugai	R. *luge*. cf. Central Nicobar *luue*. Also Palaung *uəy* and Khasian. Shorto 1437a.
throat		phiŋuāin	
throw		hekoāug	
tie		pein	
tight		diteo/ gikkai	
tired		cichā̃i	

English gloss	**PoS**	**Shom Pen**	**Commentary**
tobacco	n.	mhoy	R. *henk tjerōin.*
tomorrow	adv.	jābhu	
tongue	n.	hiɲāo	
tooth	n.	tām	
toothless	n.	tai tām	
toothache	n.	tāitaām/ nacioggutām	
toothbrush	n.	iāi pi tām	
top		koi	
touch		tāulaŋ	
touch-me-not (a plant)		nɛteāg/nāinteāg	
tree		kouāŋ/kuŋāy/uŋāi/(ko) hā̃u	
triangular		kuiou	
trousers		kainā̃g	
tuber[1] (sp. edible)		komhā̃t	
turtle		kouŋāu	
tusk of boar		(nɛ) tāi	
two		ɛo/duo	R. *ā*. cf. Nancowry *ʔã*, Central Nicobar *ã:*. Also Palaungic & Khasian. Shorto 1562. *duo* is a loan from Indo-Aryan.
U			
unfold		lāuŋ	
untie knot		bhijāy	
upper lip		kotõoi	
upper side of leaf		(na) hougou	
urine		bātoẽ	
utensil for cooking food		tiāu	
V			
vagina		ipudāo/ugāu/totoghāb	
vomit		okheāg	
W			
waist		noam/ kulā/ gigāb	
walk		kaiāg	
walk in bending posture		gitoõi	
wall		(ko) towāg	
wash		hoɔp/gikoāi	
wasp		kouāu	
water		puoi	R. *dūi* (fresh), *le-it* (salt).
water of boiled rice		moāi	
water of coconut		lāhāi	
water of green		puoinou	

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[1] It actually means creeper. Since it is used for sewing also hence the name.

English gloss	PoS	Shom Pen	Commentary
coconut			
wavy line		kakhõu	
we (1st person dual excl.)		εmāu	
we (lst person dual incl.)		εo	
wear		goāu haggau	
weep		mεd-hε̃u	
well		hā̃khlān	
west		uhā̃guɔ	
wet		meu	
whisper		gihijāg/tε̃hi jāog	
whistle		ɸõāugge	
white		kagijāu	
whole		nεnεu/ɣimāu	
wife		apāo	
wind		kāhooi	
wings		(nε/ko) nuŋāi	
woman		tijog	R. *oju*.
wood		hā̃u	
wooden carrier		(nε) toāu	
wooden nails for fixing outrigger		koteug/pein	
work		kākukhõ/mɔykho	
worm-eaten		coug/hecoug	
wound		goiāub / (ko) ceo/kopeŋāi	
wrestling		helein/ tā̃kuioi/ duŋān/tā̃ɸu	
wrinkle		(ko) ŋhā̃d	
wrist		wākāijāuŋ	
wrist wrestling		buo kugāu	
Y			
yawn		nāɸe	
yellow		kaʔugāo	
yellow leaf		laʔõõy	

Phrases	English translation	Comment
cuāg juou	long hair	
kāgujāg juou	short hair	
uduai jhopri	small hut	*jhõpḍī* < Hindi
ulgai lau	three pigs	
phugāi loe	four cloths	
ijeʔā̃ aduoi	a little rice	
duoi khānā	much food	*khānā* < Hindi
giob nuŋāceog	nail of finger of leg	
giob nuŋā tai	nail of finger of hand	
aduoi huŋāi	big snake	
kāujāg mhou	small nose	
ijeʔā̃ uduāi nie	small hut	
cuā(g) mhou	big nose	
mioŋ meāin	closed eye	
thõāku meāin	open eye	
ceog ca	my leg (coastal speech)	
waganhā̃ ca	my trousers (coastal speech)	
kāduoi jhopri	big house	(*jhõpḍī* < Hindi)
Sentences		
nā tiou	cook food (imperative)	
nā kaiug	colne (imperative)	
kaiug ũā	go there	
nā tuoid khānā	bring food	
nā taijaid	clean (imperative)	
umheāŋ khānā	(there is) no food	
mheāŋ apāo	wife (is) not	
noānce kaiug nānbe	I shall not go to nanbe (Campbell Bay)	
mheā kāog nibu	Nibu has not come	
nākāiou nāŋ	cut betel (imperative)	
nā hoɔp	clean (imperative)	
nā ɸeāo	split (imperative)	
utāg digu	Digu has not come	
mheāŋ phagu	Phagu (is) not (there)	
kāiugghe oihõ	go there	
nuām ceo	I shall eat	
o kāiāggi nānbeo	I shall not go to Campbell Bay	
o bheāg	I shall not eat	
yeai khans	Didi will bring food	
mheāŋ giu	giu (chopper) (is) not	
kaiug tiug nānbeo	I shall go to Campbell Bay	
nhoāŋhe	come here	
cahāk jābhu te iɲe	I shall go home tomorrow (coastal speech)	

5.The classification of the Shom Pen language

Two observations can be made at once; not only is the relationship between Shom Pen and Nicobarese minimal, consisting of some lower numerals and a couple of body parts, but Shom Pen shows no obvious resemblance to any other language family. However, even stranger, the only resemblances between the Shom Pen of De Roepstorff and that of Chattopadhyay and Mukhopadhyay are these same words. Man (1889) mostly resembles Chattopadhyay and Mukhopadhyay, occasionally De Roepstorff and also introduces some new lexemes. Otherwise, even allowing for transcription differences, the two languages are clearly unrelated. The circumstances for elicitation in De Roepstorff's case were less than ideal and errors can be supposed; but it is unlikely the errors would be so thoroughgoing and systematic as to result in such a complete mismatch. The alternative explanation is thus, as Man hints, that the languages spoken among the Shom Pen are so different from one another as to be mutually unintelligible and perhaps only related in the way Andamanese languages are related. The quality of data on Shom Pen groups and particularly earlier materials is such that this cannot be asserted with confidence, but it is one explanation.

This situation makes work on the languages of the Shom Pen even more imperative. Shom Pen joins the select club of language isolates in Eurasia, along with Burushaski, Nahali, Basque and Kusunda. Obscure prohibitions make it impossible for the sort of detailed work necessary to ensure quality data to be collected. Clearly, further genetic and archaeological work on the Nicobars remains a high priority. As with the Andamans, the possibility that the Shom Pen also represent a relic of early human expansion around the rim of the Indian Ocean should be seriously considered.

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Dravidian Numerals

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Numerals belong to a relatively stable part of the lexicon of almost all language families, although they are not immune to borrowing. A convincing witness to this fact is given by some of the Dravidian languages: Brahui has borrowed the numerals beginning with "4" from Persian, the other Dravidian languages from Indo-Aryan languages: Malto (from "1"!, although there are in parallel use the inherited forms for "1" and "2"), Kurukh (from "5"), Kuwi and Kui (from "3", although in Kui the inherited forms for "3" - "7" are also used in parallel), Pengo (from "3"), Kolami (from "5", besides the parallel inherited forms), Gondi (from "8", besides the inherited forms, used in parallel). On the other hand, some non-Dravidian languages, e.g. Nihali, borrowed the numerals from their Dravidian neighbours (cf. Nihali *irar* "2", *mōṭh(o)* "3", *nālku, nālo* "4" - see Kuiper 1966, 74-75).

The main purposes of the present study are to describe the inherited Dravidian numerals, to try to understand their structure to analyze their internal etymologies, and, if it is possible, to discuss their external parallels. The external comparisons should be taken in account from hypothetically related languages, from hypothetical substrata and from neighbouring languages families and isolated languages. Concerning external genetic relationship the Nostratic hypothesis (proposing a common origin of Afroasiatic, Kartvelian, Indo-European, Uralic, Altaic, and Dravidian) is accepted. Let us mention that Robert Caldwell, the founder of the comparative grammar of Dravidian languages, was one of the first scholars to speculate about a distant relationship among these language families. Special attention is devoted to the Elamite language. We know nothing about pre-Dravidian substrata in India, perhaps with the exception of Nihali, although for this language Austric affiliation seems most probable. The old hypothesis of the Dravidian-Australian relation (Müller 1882, 95-98; Caldwell 1913, 75-77, 395; recently again Dixon 1980, 236, 488-89) is reinterpreted here from this point of view: the pre-Dravidian substratum could be related to the Australian languages (Blažek 1992, 421-431). Of neighbours of Dravidian the Munda languages are especially taken in account.

The Dravidian cardinal numerals have been reconstructed and etymologized as follows:

"one"
1.1. **oru* _ (C) / **ōr* _ (V) (DEDR 990a; Zvelebil 1977, 34) = **or-* (G. Starostin).
Etymology:
1.1.1. Andronov (1994, 169) thinks that *-r-* in **oru-*/**ōr-* is secondary regarding the root **ol-* discussed below. It is really plausible to accept the influence of *-r-* of the following numeral **iru-*/**īr-*. The sequence **wo-* is not attested in Dravidian. That is why it is possible to speculate about a protoform **wol-* which is compatible with East Cushitic **wal-*/**wil-* > Saho *wili* "one", Somali *wal* "all", Elmolo *wol* "together", Oromo *wol(-i)* "together, with", Sidamo *wole* "other" (Sasse 1982, 188-89).
1.1.2. There is a hypothetical possibility to identify here a substratum influence of the Australian-like type, cf. the examples from various groups of Pama-Nyungan: Karanya *uru* (Curr 2, #104), Pitta-Pitta *ururu* (Kluge 1938, 68 after Roth) in two languages of the Pitta-Pitta group; Karuwali (Karna subgroup of the Dieri group) *orru* (Curr) = *uru*, Wongkumara (Ngura subgroup of the Dieri group) *warra* "1" (Curr II, ##106, 52; Schmidt, *Anthropos* 7, 1912, 492).
1.2. **oṉṟu* (DEDR 990d) = **on-ṯu* (Krishnamurti 2001, 255) = **onḏ-* (G. Starostin).
Etymology:
1.2.1. Andronov (1994, 168-69) reconstructs the starting point **oṉ-tu* < **ol-tu* on the basis of Tamil *ol-*, Malayalam *ollu-* "to unite", cf. also Tamil *ol* "end". The regular development *-l* + *t-* > *-ṉṟ-* may be demonstrated e.g. by the Tamil verb *al-* "to be not so-and-so", in the 3rd. sg. ntr. *aṉṟu* (DEDR 234).
1.3. **okk-* "one, single, alone" (DEDR 990b) = **ok(k)-* (G. Starostin).
Etymology:

1.3.1. Andronov (1994, 169) derives it from **ol-* + *-kk-*, via **oṟk-*, cf. Konda ***uRku uRku*** "one each".
1.3.2. Krishnamurti (2001, 255) prefers the bare root **o* + *-r-*, *-n-*, *-k-* where the root **o* had to be attested in Old Tamil **o* "to unite". Maybe, this solution is compatible with the idea of Andronov (1978, 240) who speculates about the same most primitive root reflected in Malayalam *o-* "to be similar".
1.3.3. Andronov (1978, 240) also admits a distant relationship with IE **oi̯-no/ko/u̯o-* "one".
1.4. **oṇṭi* "alone, single" (DEDR 990c).

"two"
2.1. **iru* _ (C) / **īr*_ (V) (DEDR 474; Zvelebil 1977, 34) = **ir-* (G. Starostin).
Etymology:
2.1.1. Caldwell (1913, 331) derived the numeral **ĭ̄r-* "2" from the verb **īr-*, attested in Tamil *īr (-pp-, -tt-)* "to drag along, pull, attract, carry, flay, draw, paint, write", Malayalam *īr* "splitting, sawing", *īruka* "to saw, split", Kannada *īr* "to pull, draw", Parji *īrp-* "to pull", Gadba *īr-* "to pull, drag", Gondi *ric-* "to cut with saw". But Pengo *nir-* "to pull", if related, probably excludes this etymology (DEDR 542).
2.1.2. Caldwell himself also tried to find parallels outside the Dravidian family. His comparison with the Kartvelian counterparts is undoubtedly remarkable: Georgian *or-, vor-*, Megrelian *žir-, žər-*, Laz *žu(r)-, ǯu(r)-, jur-*, Svan *jōri, jori, jerbi* < **jerwi* < **jewri* < **jori*, derivable from proto-Kartvelian **jor-* "2" (Klimov 1998, 144-45). But the fact that it is the only common Kartvelian lexeme reconstructed with initial **j-* is rather suspicious.
2.1.3. There is again a substratum alternative indicated by Australian forms: Tiwi (one of the non-Pama-Nyungan languages spoken on Melville Island north of Arnhem Land) *yurrara* "2" (Blake 1981, 112); Wailpi *yierlina* "2", Kaurna *illa* "2", *yerrábbŏla* "4" (both Yura subgroup of the Southwest group of the Pama-Nyungan family), Meyu (dialect of Kaurna) *yerra* "mutually, both", *yerrabula* "4" = 'dual of' *yerra* (Kluge 1938, 59-61).
Note: The North Munda numeral **iral-* "8", attested in Santali, Birhor *irəl*, Mundari *iralia*, Ho *irilia*, Kurku *ilar(ia)* (Pinnow 1959, 86), stands isolated within both Munda and Austro-Asiatic in general. The numeral is explainable from Dravidian, if it reflects a compound consisting of the Dravidian numeral **ir-* & **al-* "to be not so-and-so" > Tamil *al-* id., Malayalam *alla* "is not that", not thus", Kota *ala·* "to be not so-and-so", Toda *alos̱y* "except", Kannada *alla* "to be not so-and-so", Kodagu *alla* id., Gondi *hal* "not", Malto *-l-* 'negative morpheme', Brahui *all-* 'base of past negative tenses of *anning* "to be"' (DEDR 234), it means **ir-al* "two-is not". It is possible to think of a (North) Dravidian origin of this specific North Munda isogloss.
2.2. **utri* "pair" > Tulu *udri* "a match, pair", Telugu *uddi* "a match, an equal, a rival; equal", *uddincu* "to pair, match, couple" (DEDR 623).
2.2.1. In agreement with the hypothesis of a substratal influence of the Australian type it is tempting to ask, if this word could not be related to Pama-Nyungan **kut*^{y}*arra* "2" (Blake 1988, 43: formed from **kut*^{y}*a* by the non-singular suffix *-rra*; cf. the reconstruction of the pronoun of the 2nd person: sg. **NHu-* : du. **NHunpala* : pl. **NHurra*, by Evans 1988, 103). The loss of the expected initial **k-* is nothing rare in Dravidian, especially before the back vowels, cf. e.g. Tamil *kōṉāy / ōṉāy*, Malayalam *kōnāyi /ōnāyi* "wolf" (Andronov 1994, 85).

"three"
3.1. **muv* _ (C) / **mū* _ (V) (DEDR 5052; Zvelebil 1977, 34-35) = **mū-* (G. Starostin) = **muH-* (Krishnamurti 2001, 330: plus the neuter marker **-nṯu*).
Etymology:
3.1.1. Andronov (1994, 169-70) assumes the segmentation **mūṉ-* & the neuter marker **-tu*. In the first edition of his *Comparative Grammar of Dravidian Languages* (1978, 242) Andronov speculates about the derivation from Dravidian **muṉ-* > Tamil *muṉ* "in front", *muṉai* "front, face, eminence, point, edge", Malayalam *mun* "priority in space and time", Kota *mun-* "front, fore", *mon* "point", Toda *mun* "in front", *mïn* "sharp point", Kannada *mun* "that which is before, in front of", *muntu* "the front part or side, front", Kodagu *miññа* "in front, further", *mone* "sharp point", *muŋ gay* "forearm", Tulu *munè*, *monè* "point, end", Telugu *muni* "first, former, previous, front", Kolami *muni* "sting of scorpion", *mut* "before" = Naiki *mund* id., Parji *muna vanda* "forefinger", *mundi* "in front", *mūni* "tip, point", Gadba *mundēl* "the front", Gondi

munne "in front of", *mūne* "ahead", Konda *mundala* "in front", Kuwi *munu* "point of needle", Kurukh *munddh, mund* "first, ahead of, previous to", Malto *mundi* "formerly, in ancient times", Brahui *mōn* (DEDR 5020). He thinks about the development "protruding finger" = "middle finger" = "third finger". The root **mun̲-* is really used to designate one of fingers, namely "forefinger" in Parji *muna vanda*, probably in the sense "first finger" (if the "thumb" was not included).
3.1.2. Accepting an alternative, namely substratum origin, it is necessary to take in account some of the Australian forms for the numeral "3" (all Southwest group of the Pama-Nyungan family): Natingero dialect of Kalamai *mow* (Mirniny subgroup), Yungar dialect of Wadjuk *moa*, Wardand *mow*, Warrango *mowe*, Ngokgurring *mow*, Nyakinyaki *mow* (all the Nyunga subgroup). There are also longer forms in verious languages of the Southwest group: Natingero (see above) *monga*, Luritja or Kukatja *munngorra*, Bedengo *murrngul*, Jumu ('Lake Amadeus') *mun-kuripa*, and Malgana ('Gascoyne River') *mănguraba* or ('Sharkes Bay') *mangaranu* "3" (Kluge 1938, 54-55), where the second component could be identified with Nawu (Yura subgroup of the Southwest group) *karbu* "3" (Kluge 1938, 56).

"four"
4.1. **nāl* (DEDR 3655; Zvelebil 1977, 34; G. Starostin).
Etymology:
4.1.1. Following Kittel, Caldwell (1913, 335) speculated about a relation of the numeral **nāl* "4" and the adj. **nal* "good" > Tamil *nal (nar̲-)* "good", *nalla* "good, ine, excellent, abundant", Malayalam *nal* "good, fine", *nalla* "good, right, fine, handsome, real, true", Toda *nas̲* "beauty", Kannada *nal* "goodness, fairness, fineness", *nala(vu), nalivu* "pleasure, delight", Kodagu *nallë* "good", Tulu *nala, nalu̥* "good, cheap", Telugu *naluvu* "beauty, ability, beautiful", Gondi *nelā* "good" (DEDR 3610). But Caldwell himself admitted that the semantic development remains unexplained.
4.1.2. Andronov (1994, 170) mentions that Tamil *nālu* means both "4" and "several", similarly Malayalam *nālu* and Telugu *nālugu*. But the primary meaning "several" would be understandable in the case that "four" was the highest numeral. It seems more probable to suppose a secondary development "four" → "several" and not vice versa..
4.1.3. On the other hand, there are remarkable external parallels. Caldwell (1913, 335) concluded: "The resemblance between the Finnish tongues and the Dravidian, with respect to the numeral "four", amounts almost to identity, and can scarcely have been accidental." Let us mention the Fenno-Ugric data: **neljä* (> **ńeljä*) > Finnish *neljä*, Estonian *neli*, gen. *nelja* | Lappish North *njælljĕ*, Lule *nielja*, Kildin *ńielj*, Akkala *ńeľ* | Mordvin Erzya *ńiľe*, Moksha *ńiľä* | Mari *nəl* | Udmurt Kazan *ńüľ*, Sarapul *ńi̮ľ*; Komi Permyak *ńoľ*, East *nuľ* | Khanty Vach *nĕlə*, Obdorsk *ńil*; Mansi Tavda *ńiľī·*, Pelymka *niľə*, Sosva *ńila*; Hungarian *négy* id., *negyven* "40" (UEW 315-16) ||| ?Altaic: Middle Korean *nəyh* id. || Tungus **ńö[l]gün* "6" (Blažek 1999, 130).
4.1.4. Alternatively the Dravidian numeral "4" could be of substratum origin, if we accept its Australian-like affiliation, cf. the forms for the numeral "4" in some of Australian languages, all from the vast Pama-Nyungan family: Nawu (Yura subgroup of Southwest group) *nulla* (#63: 'Gawler Range' by Curr 1886), unidentified language *nalira* (#155 from 'Tambo, Barcoo River'), Koa (Maric subgroup of Pama-Maric group) *nadera* (#140: 'Diamantina River, Middleton Creek'), Maraura (Narrinyeri group) *nailko* (#84: 'Murray River'). These numerals were collected by E.M. Curr in his monumental collection of aboriginal vocabularies *The Australian Race*, I-III, published in Melbourne 1886; here quoted according to Trombetti 1923, 83 who first compared Dravidian & Australian forms).
4.2. **kirt-a* "one-fourth" > Kannada *gidda, girda, gira* "a fourth part"; *giddana, gidna* "the fourth part of a solige", Telugu *gidda, gidde* "one-fourth of the sōla measure" (DEDR 1553).
4.2.1. It is tempting to speculate about a relation with Pama-Nyungan **kut*^{y}*arra* "two" (see 2.2.). In some of the Australian languages the numeral "4" is formed from the numeral "2" by the dual suffix **-pa/ula*, cf. Potaruwutj (Kulin group of the Pama-Nyungan family) *kirtpan & kurtpun* (Curr III, 492, 494: 'Hopkins River'; quoted after Kluge 1938, 74).

"five"
5.1. **cayN _* (C) / **cay _* (V) (DEDR 2826; Zvelebil 1977, 34-35) = **śai-* (G. Starostin).
Etymology:

5.1.1. Andronov (1994, 171-72) connects the numeral "5" with Dravidian **kay-/*key-* "hand" > Tamil *kai* "hand, arm; elephant's trunk; handle", Malayalam *kai, kayyi* id., *kayyu* "the hand", Kota *kay* "hand, arm", Toda *koy* id., Kannada *kay, kay(y)i, key* "hand, fore-arm; handle; trunk of elephant", Kodagu *kay* "hand, arm", Tulu *kai* "hand; handle", Telugu *cēyi, cey(y)i* "hand, arm; elephant's trunk", *kai* "the hand", Kolami *ki·, kīy, kiyu, key* "hand, arm", Naiki *kī* "hand", Parji *key* id., Gadba *ki, kiy, kiyyū* id., Gondi *kay, kai* id., Konda *kiyu, kivu* id., Pengo *key*, Manda *kiy* id., Kui *kaju, kagu* "hand, arm; elephant's trunk", pl. *kaṣka*, Kuwi *kēyū, keyyu, keyu*, pl. *keska*, Kurukh *xekkhā* "hand, arm", Malto *qeqe* "hand" (DEDR 2023). Andronov's solution is undoubtedly acceptable from the point of view of semantics, but it is difficult to understand the conditions of palatalization of the numeral in contrast with the word "hand" (it is the main objection of Krishnamurti 2001, 255). Let us stress that only the Telugu form *cēyi, cey(y)i* "hand" is palatalized (but not *kai* "the hand"). On the other hand, it is legitimate to suppose the influence of the following numeral **caṟu/*cāṟ* "6".
2) It is tempting to speculate about relationship of the numeral "5" and Konda *sēna*, Pengo *hēni* "many" (DEDR 2824).
5.1.2. Altaic: Middle Korean *tasǎs* "5" = *tā* "all" + *son* "hand"; *suyn* "50" (Blažek 1999, 130).
5.1.3. There are interesting parallels in the Austro-Asiatic languages: geographically closest is Khasi *san* "5" (Nagaraja); cf. further proto-Mon **(m-)suun* id. (Diffloth), etc.

"six"
6.1. **caṟu* _ (C) / **cāṟ* _ (V) (DEDR 2485; Zvelebil 1977, 35) = **śāḏ-* (G. Starostin).
6.1.1. Andronov (1994, 172) rejects the proto-Dravidian age of the pattern **caṟu*_ (C) /**cāṟ* _ (V) which is limited to South Dravidian. He reconstructs the starting-point **cāl-* + *-tu* with the neuter marker **-tu*, almost universal in formation of numerals, while the bare root should be identified with **cāl-* > Tamil *cāl* "to be abundant, full, sufficient, great", Malayalam *cāla* "richly, fully", Kannada *sāl(u)* "to be sufficient or enough, suffice", Telugu *cālu* "to be able, capable, bear, endure, be enough, sufficient", *cāla* "abundant(ly)", Kolami *sāl* "to be able, can", Gondi *hālna* "completely", *āl-* "to be able", Konda *sāl-* "to be capable of, be suitable", Kuwi *hāl-* "to suffice, be enough to" (DEDR 2470). The primary meaning of the numeral "six" = "the abundant [one]" is quite natural, cf. the most probable etymology of the Indo-European numeral **(k̂)su̯ek̂s-* ~ **(s)u̯ek̂s-* "6" based on the root **u̯ek̂s-* "to grow" > Lituanian *vešė́ti* "to grow vigorously; flourish" (see Blažek 1999, 239-41 with references). For the change **-l* + *t-* > **-ṟ-* Andronov (l.c.) finds analogy in the caseless sandhi of the type Tamil *āṟīdu* "banyan [is] bad" < *āl tīdu* (Tamil *āl, ālam* "banyan", etc. [DEDR 382] & Tamil *tītu* "evil, fault, defect" [DEDR 3267]).
6.1.2. It is tempting to speculate about a compound of the type **cay* "5" + **oṟu-* "1".

"seven"
7.1. **er̤u* _ (C) / **ēr̤* _ (V) (DEDR 910; Zvelebil 1977, 35) = **eẓ-u-* / **ēẓ-* (Krishnamurti 2001, 63) = **jōr̤-* (G. Starostin: vocalization after Gondwan **jōr̤-* while **-ē-* in other branches should have been influenced by the following numeral **eṇ-* "8"; in his dissertation Starostin 2000, #350 reconstructs **ē*, i.e. **jēr̤-*, in his transcription **jēṛ-*).
7.1.1. Caldwell (1913, 342) explained the numeral as the verbal noun from the verb **er̤(u)* "to rise" > Tamil *er̤u* "to rise, ascend (as heavenly body), rise by one's own power, originate, be excited, aroused, increase, grow, begin", *er̤ucci* "rising, ascent, elevation, starting, origin, beginning, birth", *er̤umai*, Malayalam *er̤u* "height, prominence", Kota *eṛ* "weight", Kannada *er̤, ēr̤(u)* "to stand up, rise, awake, spring up, be produced, be obtained", Kodagu *ë·ḷ-* "to get up", Tulu *erkuni* "to rise or collect", *erka* "full; fullness", Brahui *harfing* "to raise, support, carry (off)", etc. (DEDR 851). Although the semantic motivation is not transparent (*"full"?; cf. Written Mongolian *doluγan* "7" : Turkic **dōl-* "to fill" or Turkic **jätti* "7" : **jät-* "be enough", Turkish dial. *yetiz* "all, whole, full"; see Blažek 1999, 116), this etymology remains most promising.
7.1.2. Caldwell (1913, 342) also speculated about a relation with Turkic **jätti* "7" (on etymology - see 7.1.1.), but there is no regular correspondence between Dravidian **-r̤-* and Turkic **-t(t)-*.
7.1.3. There are other possible cognates in Altaic: Turkic **jȫŕ* "100" | Written Mongolian *yerü* "the most of", *yerüdügen* "for the greatest part, generally", *yerüŋkei* "common" | Middle Korean *jə́r* "10", *jə̀rə́h* "a big quantity, number" | Old Japanese *jòrò-du* "10.000" (see Blažek 1999, 117). The primary semantics is

in good agreement with the Dravidian verb **er̤(u)* "to rise" etc., discussed in §7.1.1. For both Altaic and Dravidian a common Nostratic denominator **jeŕU* may be established.

Note: The North Munda numeral **eya* "7", attested in Santali *eae*, Mundari *eja*, *ea(e)*, Ho *iya*, *aia*, Birhor *eae*, *aea*, Kurku *(y)eya* (Pinnow 1959, 269), stands isolated within Munda and Austro-Asiatic at all. It could be borrowed from a Dravidian donor-language changing Dravidian *-*r̤*- into -*y*-. This change is described e.g. in Irula & Kota (*kuyi* & *kōy* "pit, hole" : Tamil *kur̤i* [DEDR 1818]); Toda *tōy* "to be lowered" : Tamil *tār̤* id. [DEDR 3178]; Manda (*nuy*- "to wash" < **nor̤-/*noṭ-* [DEDR 3783]); Kurukh (*kiyyā* "under", also Brahui *kī* "below, down" < **kīr̤-* [DEDR 1619]), cf. Zvelebil 1970, 150, §1.34.3-4; 151, §1.34.5; 153, §§ 1.34.11.3 & 1.34.14; Krishnamurti 2001, 42-75. Just the North Dravidian or better its ancestor, still preserving the higher numerals, is a good candidate for a donor-language into the North Munda branch.

"eight"

8.1. **eṭṭu* / **eṇ* (DEDR 784; Zvelebil 1977, 35) = **eṇṭu* /**eṇ* (Andronov 1994, 173) = **eṇ-* *(-nṭ-)* (G. Starostin).

8.1.1. It was already Caldwell (1913, 345-46) who identified here the base **eṇ* "number", **eṇ-* "to count, calculate", plus the neuter suffix **-(t)tu*; cf. the Dravidian data: Tamil *eṇ* "thought", calculation, number", *ēṇi* "number", *eṇṇu* "to think, consider, determine, esteem, conjecture, count, reckon", Malayalam *eṇ* "number, thought", Toda * öṇ-* "to count", *öṇm* "counting, numbers", Kannada *eṇike* "counting, number, thinking, observation", *eṇṇu* "to count, think", Kodagu *ëṇṇ-* "to say, tell", Tulu *eṇṇuni* "to count, think", *eṇṇige* "calculation, estimation", Telugu *ennu* "to count, reckon", *encu* "to count, reckon, enumerate, think, consider, believe", Parji *eja* "number", *eja cāj-* "to count", Pengo *eja ki-* id., Manda *eji ki-* id., Kuwi *eji kīnai* id. (DEDR 793).

8.2. **eṇ(u)-pa(k)ti* is reconstructible for Tulu, Telugu, Kolami and Gondi.

8.2.1. Although Tyler (1986, 10) accepts the identification of the word **eṇ* "number" with the numeral "8", alternatively he offers to interpret the compound **eṇ(u)-pa(C)ti* as "a pair [subtracted from] ten" or "rest of ten", where the first component could be identified with Tamil *ēṉai* "other; the rest" (DEDR 919) or **iṇay* > Tamil *iṇai* "pair, couple, likeness, union", Malayalam *iṇa* "pair, couple, union, companion", Kannada *eṇe*, *eṇa* "a couple, pair, connexion, equality, similarity, a match", Kodagu *ëṇe* "double", *ëṇe makka* "twins", Tulu *inè*, *iṇè* "a couple, pair, companion, mate", Telugu *ena* "equal, equality, a match" (DEDR 457). Although it is possible to imagine these constructions, the phonetic problems **i* vs. **e*, **ṉ* vs. **ṇ* remain unresolved.

8.2.2. Andronov (1994, 173) judges that the second component was used secondarily in analogy with the numeral "9".

"nine"

9.1. **oṉ-/or-paktu* (DEDR 1025).

9.1.1. Apparently "one [subtracted from] ten" (so Caldwell 1913, 347, and independently Gundert).

9.2. **toḷ-(pak-)tu* (DEDR 3532) = **toḷ-pad-* (G. Starostin).

9.2.1. Caldwell (1913, 348) supposed that **toḷ-* should have been identified with Dravidian **tol-/*toḷ-* "before", directly attested in Kuwi *tolli(e)*, *toli* "before, formerly", cf. further Gondi *tollē* "previous", Telugu *toli* "beginning; first, former, previous, old", Tamil *tol* "old, ancient", *tollai, tor̤ḷai* "antiquity, ancientness" (DEDR 3516). The semantic motivation "before [ten]" is quite acceptable.

9.2.2. Almost generally accepted is identification of the first component of the numeral with the root **toḷ-*, attested in Tamil *toḷ* "to perforate, bore with an instrument", *tor̤ḷai* "hole, perforation, pit, anything tubular, fault, defect", Malayalam *toḷḷa* "hole, cavity", Kota *toyḷ-* "to pierce", *toḷ* "hole, vagina", Kannada *toḷe* "hole, cavity", Tulu *toluvè* "hole", *toḷu* "hole; empty", Telugu *toli* "hole", Gondi *tullānā* "to be pierced" (DEDR 3528; cf. also Andronov 1994, 174). The semantic motivation "defect [ten]" or "empty [ten]" seems rather vague.

"ten"

10.1. **pak-tu* (DEDR 3918) = **paH-* & neuter suffix **-tu* (Krishnamurti 2001, 328) = **paT-* (G. Starostin).

10.1.1. Caldwell (1913, 351) accepted the idea of Gundert about a borrowing of the Dravidian numeral "10" from Sanskrit *paṅktí-* "set of five", cf. also the ordinal numeral *pakthá-* "fifth". The divergence of South Dravidian, Telugu, and the branches represented by Kolami and Gondi is dated to the middle of the 2nd cent. BC by Andronov (1994, 13), or even later, to c. 1200-1000 BC by (Starostin 2000, 217-18). In any case, the contact of Indo-Aryans and ancestors of those Dravidians, who preserved this numeral, was possible.
10.1.2. Andronov (1994, 176) proposes a long chain of changes **pax-* < **pav-* < **pam-* < **paṉ-* < **pal-*, to identify here the root **pal-*, attested in Tamil *pala* "many, several, diverse", *paṉmai* "plurality, multitude", Malayalam *pala* "many, several, various, Toda *pes̱* "a large number, many", Kannada *pala, palavu* "much, many, several, various", *palavar* "several persons", Telugu *palu* "many, several, various, different", *paluvuru, paluvuṇḍru* "many persons", Malto *palware* "to be multiplied, be bred" (DEDR 3987). Semantically this idea is acceptable, but the series of proposed transformations is rather long.
10.1.3. Caldwell (1913, 353) offered another possible internal etymology, namely based on the idea "duplicity" ("10" = "2x5"?) or "share" ("$^1/_{10}$"?), cf. Tamil *pakku* "fracture, duplicity", *pāttu* "dividing, sharing, share, half, *pātti* "division, section, part, share", *paṅkam* "portion, division", Toda *paxy* "division", Kannada and Tulu *pagadi* "tribute, tax", etc., all from the verb **pak-* "to divide / be divided" (DEDR 3808).

"hundred"
11.1. **nūṟ(-tu)* (DEDR 3729) = **nūḏ-* (G. Starostin).
11.1.1. Caldwell (1913, 354) sought a starting-point in Tamil *nūṟu* "to crush, pulverize, reduce to powder; powder, dust, flour", Malayalam *nūṟu* "powder", etc. (DEDR 3728).
11.1.2. Menges (1968, 97) sought a cognate in Turkic **jǖŕ* "100", but Dravidian **n-* is compatible with Turkic **j-* only when the latter is a reflex of Altaic **ń-* (> Mongolian **n(i)-*, Tungusic **ń-*). On the other hand, Turkic **-ŕ-* indicates as a regular counterpart Dravidian **-r̤-* (Illič-Svityč 1971, 150; 170). The cognates in other Altaic languages exclude the genetic relationship of these numerals, cf. Written Mongolian *yerü* "the most of", *yerüdügen* "for the greatest part, generally", *yerüŋkei* "common" | Middle Korean *jə́r* "10", *jə̀rə́h* "a big quantity, number" | Old Japanese *jòrò-du* "10.000" (see Blažek 1999, 117).

"thousand"
12.1. Telugu *vēyi, veyi, veyyi*, pl. *vēlu* "1000", *vēna-vēlu* "thousands by thousands".
12.1.1. Derived from the root attested in Tamil *viyam* "extensiveness, height", *viyal* "greatness, width, expansion", *viyaṉ* "greatness, vastness, excellence", Malayalam *viyam* "extension", Gondi *weeya* "high" (DEDR 5404).

Appendix 1: What is known about Elamite numerals?

The hypothesis of a close Elamite-Dravidian relationship was first formulated by Caldwell (1856; cf. 1913, 57, 65), later also Trombetti, Bork, Diakonoff and McAlpin tried to demonstrate it. In the case of a really close relationship one would expect some cognates between numerals. Let us mention what we know about the Elamite numerals according to interpretation of Hinz & Koch, the authors of the monumental *Elamisches Wörterbuch* (1987):

ki "1" (and with various extensions *kik, kikki, kikqa*; with the personal marker *kir*) - see EW 459, 465, 467, 468-69.
mar "2" (and the variants syllabically written *ma-ir, mar-ra*) - see EW 876, 860.
ziti "3" - see EW 1305.
tuku "5"? - see EW 356.
barba "80"? - see EW 147.

It is apparent that among known Elamite numerals there are no cognates in the system of Dravidian numerals (maybe with exception of *mar* "2", cf. Dravidian **maṟu* "other, next, again" [DEDR 4766]). Let us also mention that König (1965, 42, fn. 15) offered to interpret Middle Elamite *nulkippi* "4 pairs" = "8". If his solution was correct, the hypothetical root **nul-* should be a cognate of Dravidian **nāl* "4". But Hinz & Koch (EW 1016) interpret it quite differently, in *nulkippi* they see the plural of 'fertility-maker'.

Appendix 2: A survey of the Dravidian inherited cardinal numerals

_(C) / _(V)	**oru / *ōr* "1"	**oṉṟu* "1"	**oṇṭi* "alone"	**okk-* "1"	**iru / *īr* "2"	**muv /*mū* "3"
DEDR (#)	(990a)	(990d)	(990c)	(990 b)	(474)	(5052)
Tamil	*oru* (C) / *ōr* (V)	*oṉṟu* nr. one; n. *oṟṟai* one of a pair, odd number	*oṇṭi* that which is single, one who is alone; *oṭṭai* one of a pair	*okka* together	*iraṇṭu*; adj. *iru* (C) / *īr* (V) *iru-p/vatu* 20 *iruvar* 2 men *iraṭṭai* pair	*mūṉṟu* & *mūṇ u* adj. *mu* (CC) / *mūv* (V) *mu-ppatu* 30 *mūvar* 3 men
Malayalam	*oru / ōr*	*onnu* n., *oṟṟa* one, single, odd		*okka* together	*raṇṭu*; adj. *iru* (C) / *īr* (V) *iru-patu* 20 *iruvar* 2 men *iraṭṭa* double	*mūnnu* *mu-/nu-ppatu* 30 *mūvar* 3 men
Kasaba		*o(y)ndu / oṉṉu*			*reṇḍu*; *irppattu* 20	*mūyndu* *mūvattu* 30
Kurru		*oṇḍu / uṇḍu*			*reṇḍu / roṇḍu* *iravay / iridi* 20	*mūṛu*
Irula					*raṇḍu / reṇḍu*	
Kota	*or, o·r, o*	*oḏ* 1 *pan onḏ* 11			*eyḏ* *ir va·d* 20	*mu·nḏ* *muat* 30
Toda	*wï·r, oš, o*	*wïḏ*	*waṭy* single, odd < Ta *oṭṭai*		*e·ḍ*; *i·r o·ṛ* 2 years *ï foθ* 20 *ïm* double	*mu·ḏ* *mu poθ* 30
Kannada	*or* (C) / *ōr* (V)	*ondu* 1 thing	*oṇṭi* 1, alone		*er(a)ḍu, erar̤* adj. *ir(u), ik, ic* *irpattu* 20 *irbar* 2 men	*mūṟu* adj. *mu(k), mū* *mū-vattu* *mūvar* 3 men
Koḍagu	*orï*	*ondï* 1 thing *pannandï* 11		*okka* together	*daṇḍï* n. *iru-vadï* 20 *ibba* 2 men	*mu·ndï* *mu·vë* 3 men *nuppadï* "30"
B.Kuṟumba		*-oṇḍe*			*-əḍḍu*, adj. *əḍ*	*-mu:ṟʉ*
Šōlega		*oṇḍu/o*			*eruḍu/o* *ippattu* 20	*mu:ṟu/o* *mu:vattu* 30
Tulu	*or* adj., *oru* single	*oñji* 1 thing ondikè joining	*oṇṭi* alone, single		*raḍḍu̥* n. adj. *iru̥, ir-* *irva* 20 *irveru̥* 2 men	*mūji* *muppa* 30 *mūv(v)eru̥* 3 men
Koragu		*oñji / uñji*			*eyḍi*	*mūji*
Telugu	*orumu* be united	*oṇḍu* 1 thing	*oṇṭi* singleness	*oka* 1, single	*reṇḍu* n. *iru-, īr-* comp. *iruva(d)i* 20	*mūḍu*, inscr. *mūnṟu* adj. *mu, mū*

					ibbaru 2 men	*muppa(d)i* 30 *mūvuru* 3 men
Kolami				*okkon* m. *okkod* f. *ok maṭ* once	*indiŋ* 2 things *iddar* 2 men *i·ral* 2 women *irve* 20	*mu·ndiŋ* 3 things *muggur* 3 men
Naiki				*okko(n)* m. *okko(d)* f./ n. *okoḍa* once	*indiŋg* 2 things = Ch. *ernḍi* *iddar* 2 men = Ch. *iroṭer* *iraḷ* 2 women = Ch. *ira*	*mūndiŋ* 3 things = Ch. *mūndi* *muggur* 3 men = Ch. *mug(g)ur* *muyaḷ* 3 women = Ch. *muy(y)a*
Parji				*ok* adj., *okur* m., *okal* f.	*ir* adj. *irḍu* 2 things *irul* 2 men *iral* 2 women	*muy* adj. *mũ:duk* 3 things *mūvir* 3 men *muyal* 3 women
Gadba				*ukur* m., *okuṭ* f., *ukuṭ* n.	*iḍḍig* n., *ir* adj. *inḍi* 2 things *irul* 2 men *iral* 2 women	*mūṇu* *mūnḍ* n. *muvur* m. *muyal* f.
Gondi	*oror*, *oṛe* m.	*unḍi*, *uṇḍī*, *undi*	*unṭhal* n.	*ōkō* one each	*ranḍ(u)* non-m. *irvur/ṛ* m. *ranṭe* pair	*mūnḍ* non-m. *mūvir* m. *muhk* 3 each
Konḍa	*or-*	*unṟi* f. / n.			*ri*$^{(ʔ)}$- *ri'er* 2 men *runḍi* f./ n.	*mūnṟi* n. *mu'er* 3 men *mūRku mūRku* 3 each
Pengo	*ro*, *ronje*				*ri*; *rindaŋ* n. *rikar* 2 men *rinḍek* 2 women	
Manḍa	*ru*, *runḍi*				*ri*; *rikar* 2 men *rikehiŋ* 2 wome	
Kui	*ro*, *ronḍi* n. *roanju* m.				*rī*; *rīnḍi* f./ n. *ri'ari* 2 men *ri kōṟi* 40	*mū:*; *mūnji* f./ n. *mu'ar* 3 men *mū kōṟi* 60
Kuwi	*rō*; *ro'esi* 1 man *roṇḍi* 1 woman				*rī*, *ri'ari* 2 men *riṇḍi* f./ n.	
Kurukh	*or*^{o}*t* m., *ot*^{o}*x* f. *ormā* all	*ōn(d)* one whole	*oṇṭā* 1 thing	*oŋghon* once	*ẽ:ṛ*, *ēṇḍ* 2 thing *irb* 2 men	*mūnd* 3 things *nubb* 3 men
Malto	*ort*, *-e* m., *-i* f. *orme* all	*-ond* 1 thing			*-is* 2 things *iwr* 2 men	
Brahui	*asiṭ* 1 (entity) *asi* adj. *asīke* once				*iraṭ* 2 (entities) *irā* adj.	*musiṭ* 3 (entities) *musi* adj. *musīka* thrice

_ (C) /_ (V)	**nāl* "4"	**cayN* / **cay* "5"	**caṟu* / **cāṟ* "6"	**er̤u* / **ēr̤* "7"
DEDR	(3655)	(2826)	(2485)	(910)
Tamil	*nāl(u)*, *nālku*, *nāṉku* 4 *nāṟpatu*, *nāppatu* 40 *nālvar* 4 men	*aintu*, *añcu*; adj. *ai* 5 *aim-patu* 50 *aivar* 5 men	*āṟu* 6 *a̱ru-patu* 60 *aṟuvar* 6 men	*ēr̤u* 7 *er̤u-patu* 70 *er̤uvar* 7 men

Malayalam	*nāl(u)*, *nāṅku*, *nān* 4 *nālpatu* 40 *nāl(uv)ar* 4 men	*añcu*; adj. *ai* 5 *am-patu* 50 *aivar* 5 men	*āṟu* 6 *aṟu-patu* 60 *aṟuvar* 6 men	*ēṛu* 7 *eṛu-patu* 70 *eṛuvar* 7 men
Kasaba	*ṉālu*; *ṉalvattu* 40	*añju*; *ayvattu* 50	*āru*; *aṟavattu* 60	*ēḷu*; *eḷuvattu* 70
Kurru	*ṉālu*; *ṉalabay* 40	*añju*; *yābay* 50	*āru*; *aravay* 60	*ōgu*; *ḍabbay* 70
Kota	*na·ng*; *na·n digl* 4 months; *na·lvat* 40 *na··r pa·d* 40 days	*anj* 5 *ay vat* 50	*a·r* *ar vat* 60	*e·y*, dial. *e·g* *eḷ vat* 70
Toda	*no·ng*; *pa·ng* 14 *nał poθ* 40	*üz*, dial. *üj*; *pu·j* 15 *e boθ* 50	*o·ṟ*; *pa·ṟ* 16 *aṟoθ* 60	*öw*; *pa·w* 17
Kannada	*nāl(u)*,*nāl(u)ku*, *nā(ku)* *nālvattu* 40 *nālvar* 4 men	*ay(i)du*; adj. *ai* *ay-vattu* 50 *aybaru* 5 persons	*āṟu* *aṟu/a-vattu* 60 *aṟuvar*, *ārvaru* 6 men	*ēṛu* *ēṛ-pattu* 70 *ēṛvaru* 7 men
Koḍagu	*na·lï*; *na· padï* 40 *na·vë* 4 men	*añji*; *aim-badï* 50 *ayvë* 5 men	*a·rï*; *aru-vadï* 60	*ë·ḷïï*; *ëḷu-vadï* 70 *ëvvë* 7 men
B.Kuṟumba	*na:kʉ*	*əyidu*	*a:rʉ*	*ö:ḻʉ*
Šōlega	*na:ku/o*; *na:lvattu* 40	*ayidu/o*; *aivattu* 50	*a:ṟu/o*; *aṟuvattu* 60	*ō:ḷu/o*; *yappattu* 70
Tulu	*nālu̥*; *nālpa* 40 *nālveru̥* 4 men	*ainu̥* 5 things; *aiva* 50 *aiveru̥*, *aiyeru̥* 5 men	*āji*; *āj(i)pa*, *ājip(p)a* 60	*ēḷu̥*, *ĕ̄ḷpa*, *erpa* 70 *ēḷveru̥* 7 men
Koragu	*ṉālu*	*ayṉu*		
Telugu	*nālugu*, *nāluvu* *naluvadi*, *nalubadi* 40 *naluguru*, *naluvuru* 4 men	*ayidu*, *ēnu* 5 (things) *ẽ:badi*, *yābhai* 50 *ayidu-guru*, *ēguru*, *ēvuru* 5 men	*āṟu* *aṟu-vadi*, *aruvai* 60 *āṟuguru*, *āṟuvuru* 6 men	*ēḍu*, inscr. (7th c.) *ēṛu* *ḍebbadi*, *ḍebbhai* 70 *ēḍuguru* 7 men
Kolami	*na·liŋ* 4 things *nalgur* 4 men *nallav* 4women	*aid & ayd* *segur* 5 men	*ār* *ārgur*	*eḍ*, *ēṛ* *ergur*, *eḍgur* 7 men
Naikṛi	*nāliŋ* 4 things Ch. *nāli* non-m. *nalgur* 4 men *nallaḷ* 4 women	Ch. *sēndi*	Ch. *sādi*	
Parji	*nālu(k)* 4 things adj. *nel*; *nelvir* 4 men *nelal* 4 women	*cẽ:du(k)* 5 things adj. *cem/n*; *cēvir* 5 men *ceyal* 5 women		
Gadba	*nalgur*, *nalvur* m. *nandal*, *naṇḍal* f. *nālig*, *nālug* n.			
Gondi	*nālung*; *nālk* 4 each *nāl-vir/-vur/-gur* 4 men *nāluŋ*, *lālū* non-m.	*saiyung*, *sīyuŋ*, *hayuŋ*, *ayŋ* non-m., *sey(y)ur*, *sīvir*, *(h)ayvur* m., *saik saik* 5 each	*sāṟung*,*harung/ṃ*, *ārū* *sārvir/-vur*, *(h)ārvur* m., *sarne* sixth day	*(y)ēṛung*, *yeḍung*, *ēṛū:* non-m., *ēṛ-vur/-vir*, *ēṛ(v)ur* m. *yēṛk* 7 each
Konḍa	*nāl'er* m., *nālgi* non-m.			*ēṛu*
Kui	*nal*; *nǟlgi* non-m. *nālur* 4 men	*siṅgi* 5 things, *sēngi* f./n., *sēŋ gōṛi* 100	*saj*, *sajgi* 6 things dial. *hāja* 6	*oṛi*; *oḍgi* 7 things *oṛi gōṛi* 140
Kurukh	*nāx* 4 things *naib* m./ f.			

_ (C) / _ (V)	**eṭṭu / *eṇ* "8"	**oṉ-/or-paktu* "9" **toḷ-(paC-)tu*		**pak-tu* "10"	**nūṟ-tu* "100"
DEDR	(784)	(1025)	(3532)	(3918)	(3729)

Tamil	*eṭṭu*; *eṇ-patu* 80 *eṇmar*, *eṇvar* 8 men	*oṉpatu*, *ompōtu*	*toṇṭu* 9 *toṇ-ṇūṟu* 90 *toḷḷ-āyiram* 900	*pattu*, *paḵtu*; *paḵ-pattu* 100 = 10 x 10 *patiṉ-* 10 in teens *-patu* 10 in tens *patiṉmar* 10 men	*nūṟu*, obl. *nūṟṟu-* *nūṟṟuvar* 100 men
Malayalam	*eṭṭu*; *eṇ-patu* 80 *eṇmar* 8 men	*ompatu*	*toṇ-ṇūṟu* 90 *toḷḷ-āyiram* 900	*pattu*, obl. *patin-* *-patu* in tens *patinmar* 10 men	*nūṟu*, obl. *nūṟṟu-* *nūṟṟu/avar* 100 men
Kasaba	*eṭṭu*; *embattu* 80	*ombattu* 9	*tombattu* 90	*pattu*	
Kurru	*oṭṭu*; *eṉ(a)bay* 80	*ombedu* 9		*pot(t)u*	
Kota	*eṭ*, *em bat* 80	*orba·d*, dial. *onba* 9 *patrmba·d* 19	*tombat* 90	*pat*; *paṉḏ* 11, *pad-* in 13, 14, *padn-* in 15-18 *-vat* /*-bat* in some tens	*nu· r*
Toda	*öṭ*, *pu·ṭ* 18	*wïnboθ*, *pu·ṇboθ* 19 *e·ṇ boθ* 90		*pot*; *pon-* in 11-13; *-(f)oθ/-poθ/-boθ* in tens	*nu·ṟ*
Kannada	*eṇṭu*; *eṇ-pattu* 80 *eṇbar* 8 men	*omb(h)attu*	*tom-b(h)attu* 90	*pattu*; *padin-* teens in 15-18; *padi-* in 13, 14; *pann-* in 11, 12; *-vattu* / *-vattu* / *-battu* in tens *padimbar* 10 men	*nūṟu* *nūṟ(v)ar* 100 men
Koḍagu	*ëṭṭï*; *ëm-badï* 80	*oymbadï*	*tom-badï* 90	*paṭṭï*; *padïn-* in 14-18, *padi-* in 13, *pann-* in *pannandï* 11, *panneraṇdï* 12 *-vadï/-padï/-badï* in tens	*nu·rï*, obl. *nu·iṭ-*
B.Kuṟumba	*-öṭṭʉ*	*-embadʉ*		*-pattʉ*	
Šōlega	*eṇṭu/o* 8; *yambattu* 80	*ombattu* 9; *tombattu* 90		*attu* 10	*nu:ṟu*
Tulu	*eṇ(u̥)ma* 8; *eṇpa* 80	*ormba*	*soṇpa* 90	*pattu̥* 10; *patt-* in 11, *padu̥-* in 12-14, *padu̥n-* in 15-19, *-va/-pa*, obl. °*tu̥* in tens	*nūdu*, obl. *nūta-* 100
Telugu	*enimidi* 8; *enu-badi*, *enabhai* 80 = inscr. *eṇ(um)bodi*; *enamaṇḍru* 8 persons		*tommidi* 9 *tombadi*, *tombhai* 90 *tomma(n)nūṟu* 900 *tommaṇḍuguru*, *tommaṇḍru* 9 persons	*padi*; *padun-* in 11, 15-18 *padu-* in 13, 14, *pan-* in 19; *-vadi/-padi/-badi* in tens inscr. *padunṟu* 10 persons	*nūṟu*, obl. *nūṭa-* *nūṟug/vuru* 100 men
Kolami	*enumadī*, *enumidī* 8 *enmāṭar* 8 persons		*tomdī̆* 9 *tomāter* 9 men	*padī*, *padi*	
Gondi	*aṛmur/l*, *yermud*, *enmīdī*, *tenmidi* 8 *aṛmuhk* 8 each	*eḍmu*, *unmā* 9 *unmāk* 9 each	*tomidi*, *tumīdī* 9 *tombai* 90	*pad(i)*, pl. *patk*	*nūr*, pl. *nuhk*

Conclusion

The results of the present study are summarized in the table:

Form	Internal etymology (§§)	External parallels

*oru /*ōr "1"		A: PN _*urr-/*warr-_ 1 (1.1.2.) N: EC _*wal-/*wil-_ together (1.1.1.)
*iru /*īr "2"		A: Kaurna _yerra_ both, Tiwi _yurarra_ 2 (2.1.3.) or N: K _*jor-_ 2 (2.1.2.)
*utr-i "pair"		A: PN _*kut^{y}arra_ 2 (2.2.1.)
*muv /*mū or _*mūn-_ "3"	_*mun̲_ front, point (3.1.1.)	A: PN (SW) _*mow_ 3 (3.1.2.) A: PN (SW) _*muṇ-kurV(ba)_ 3 (3.1.2.)
*nāl "4"		A: PN: Nuwa _nulla_; Maraura _nailko_ 4 (4.1.4.) N: FU _*neljä_ 4 ‖| AL: MK _nəyh_ 4 (4.1.3.)
*kirt-i "$^1/_4$"		A: PN _*kut^{y}arra-pula_ 4 = dual of 2 (4.2.1.)
*cayN /*cay "5"	_*kay/*key_ hand (5.1.1.) or CD _*cēn-_ many (5.1.2.)	N: AL: MK _tasǎs_ 5, _suyn_ 50, _son_ hand (5.1.2.) AU: Khasi _san_ 5 (5.1.3.)
*car̲u /*cār̲ "6"	< _*cāl-tu_ : _*cāl_ be abundant (6.1.1.)	
*(j)ẹṛu /*(j)ēṛ "7"	_*ẹṛ(u)_ rise (7.1.1.)	
_*eṇ_ "8"	_*eṇ_ number; calculate (8.1.1.)	
*on̲-/*or-paktu "9"	one [subtracted from] ten (9.1.1.)	
*toḷ-(pak-)tu "9"	[one] before ten (9.2.2.)	
*pak-(tu) "10"	Tamil _pakku_ duplicity (10.1.3.)	
*nūr̲-(tu) "100"	_*nūr̲u_ powder (11.1.1.)	
Telugu _veyyi_ "1000"	Tamil _viyan_ greatness (12.1.1.)	

Abbreviations: A Australian, AL Altaic, AU Austroasiatic, CD Central Dravidian, EC East Cushitic, FU Fenno-Ugric, K Kartvelian, MK Middle Korean, N Nostratic, PN Pama-Nyungan, SW Southwest.

1) For the numerals "6"-"10", "100", "1000", internal Dravidian etymologies seem most probable. Maybe the same can be said about the numeral "5", if its relation with the word "hand" is correct.
2) Some of the numerals lost in the North Dravidian languages ("7", "8") can be reconstructed on the basis of their probable borrowings in the North Munda languages.
3) The most surprising results concern the numerals "1" - "4". Confronting the possible cognates from other Nostratic families with Australian parallels (which would indicate hypothetical substratum origin), the latter comparanda seem more convincing.

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UEV *Uralisches etymologisches Wörterbuch*, ed. by Károly Rédei. Akadémiai Kiadó 1986-88.

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The Horse, the Wheel, and Language: How Bronze Age Riders from the Eurasian Steppes Shaped the Modern World, by David W. Anthony. Princeton University Press. 2007. 533 pages, maps, photos, drawings, tables, notes, appendix, bibliography, index.

Reviewed by Daniel F. McCall,
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In the late 18th century a British judge in India discovered a set of problems. As noted by Anthony (p.7), the judge, Sir William Jones, wrote: "The Sanskrit language...Greek...[and] Latin...[bear] a stronger affinity, both in roots of verbs and in forms of grammar, than could possibly have been produced by accident, so strong indeed, that no philosopher could examine them all three, without believing them to have sprung from some common source, which, perhaps, not longer exists." Germanic, Slavic, Celtic and even Hittite, not even known at the time of Jones' announcement, and other branches were eventually also included as descendants from this 'common source'.

Demonstrating this presented a number of interrelated problems: What was that ancestral language, how long ago did it exist, where was it located geographically, and how did its descendants spread by several bifurcations to cover such a great swath of the globe? These problems have engaged the efforts of generations of scholars in the new discipline of linguistics which developed largely from Jones's discovery. The reason Jones specifies a "philosopher" to examine the lexical and grammatical examples he had assembled was that at the time most modern sciences had not yet extracted themselves from philosophy, which remained the general activity of intellectuals. Isaac Newton had said, "Philosophy is a stern mistress." Philosophy was the search for truth and it was beginning to give birth to new disciplines of science, and linguistics joined chemistry and physics as scientific fields of investigation. In mid-nineteenth century the hobby of barrow-digging became organized to excavate old residences and burials in the careful and systematic methods of archaeology, and some archaeologists became involved with some aspects of the quest for evidence of the where and when of Proto-Indo-European, as the *common source* language is now called.

Quantities of data have been accumulated by both of these historical disciplines. Interpretations and hypotheses, which it is argued are sustained by bits of data, have been put forward, but the divide tends to remain between the two disparate disciplines. Anthony recognizes that "Many archaeologists, accustomed to digging up real things, have a low opinion of those who merely reconstruct hypothetical phonemes – what is called 'linguistic prehistory'. There are reasons for this scepticism. Both linguists and archaeologists have made communication across the disciplines almost impossible by speaking in dense jargons that are virtually impenetrable to anyone but themselves...[M]ost archaeologists, including this author, are outsiders in linguistics." (p.21). All of

Part One (120 pages) is Anthony's summation of the pertinent uses of linguistics; that is an intrepid undertaking.

The interaction of linguistic and archaeological analyses and theories has already been assessed from both camps in the now abundant literature. The handicap is the disparity of the materials of the two sciences: vocabulary, cognates and syntax on one side and artifacts, strata and radiocarbon on the other. "Most archaeologists," Anthony notes (p.17), "believe it is impossible to equate prehistoric language groups with archaeological artifacts, as language is not reflected in any consistent way with material culture. People who speak different languages might use similar houses or pots, and people who speak the same language can make pots or houses in different ways." Despite this, Anthony adds: "But it seems to me that language and culture are predictably correlated under some circumstances. Where we see a *very clear* material-cultural frontier – not just different pots but also houses, graves, cemeteries, town patterns, icons, diets, and dress designs – that *persists* for centuries or millennia, it tends to be a linguistic frontier. This does not happen everywhere. In fact, such *ethno-linguistic* frontiers seem to occur rarely. But where a robust material-culture frontier does persist for hundreds, even thousands of years, language tends to be correlated with it. This insight permits us to identify at least some linguistic frontiers on a map of purely archaeological cultures, which is a crucial step in finding the Proto-Indo-European homeland." Thus, neatly, Anthony separates himself from his more skeptical colleagues in the anthropological profession, and takes a stand for the special (though not necessarily unique) case of Proto-Indo-European (hereafter PIE). See his map: Figure 5.1. "The Proto-Indo-European homeland between 3500-3000 BCE."

Strangely Anthony does not recognize as significant a conjunction of a particular association of different types of artifacts from a particular material-culture excavation with the reconstruction of names in a proto-language for particular artifacts in a language that may have been spoken, supposedly, by the people who made the artifacts – when such a correlation occurs. Perhaps he, as well as his skeptical colleagues, retains some disdain for 'hypothetical phonemes' and 'linguistic prehistory'?

His reticence to embrace linguistic reconstruction is indicated when he comments on Robert Hall's reconstruction of the source of the Romance languages which compares clearly with extant Latin texts from Roman sources; he comments: "Such clever exercises aside, the best proof of the realism of reconstruction lies in several cases where linguists have suggested a reconstruction and archaeologists have subsequently found inscriptions that proved it correct." Of course these "best proof" cases are unassailable, but can one really use them to downgrade the 'clever exercises' that demonstrated the same thing?

Anyone who ventures, after two centuries of accumulating discourse and disputes, into a tangle of conflicting data and deductions deserves respect for audacity, but also – if some clarification results – we owe thanks. Anthony has produced a carefully constructed presentation of a plausible sequence of reconstructed events (evidenced by supporting data) to explain the history of the origin and spread of the Indo-European language family (IE). Thank you, Professor Anthony.

"The speakers of Proto-Indo-European were tribal farmers who cultivated grain, herded cattle and sheep, collected honey from honey bees, drove wagons, made wool or felt textiles, plowed fields at least occasionally or knew people who did, sacrificed sheep, cattle, and horses to a troublesome array of sky gods. These traits guide us to a specific kind of material-culture…" (p.98) Thus we move from reconstructed language to archaeologically revealed material-culture, and achieve localization of the speakers of the proto-language.

Probably some adherent to one of the other places that have been suggested for the origin of PIE (of which there are many) will criticize Anthony's location of that process, but in the last couple of decades it seems that the North Pontic-Caspian steppe, specified by Anthony, had become increasingly popular with those scholars who follow the discussion. However, Anthony in HWL does not merely join an emerging consensus; he adduces several constructs of supporting evidence: abraded horse teeth which point to putting bits in the mouths of horses to control them while riding; the use of 'elite recruitment' to switch language use to IE; a cluster of radiocarbon dates for successive stages, and a trade route for selling horses, wool, and cannabis from the steppe to the urban states to the south in Mesopotamia and Anatolia, leading eventually to invasions by steppe bands of chariot warriors.

The Neolithic economy, producing pottery, polished stone tools, grain agriculture and animal husbandry had spread through Asia Minor (present day Turkey) into southeastern Europe (the Balkans, the lower Danube region and the coastland north of the Western Black Sea). This Neolithic could not expand further east along the northern coast of the Black Sea because the drier steppes in that direction would not support agriculture. 'Tripolye' is the archaeological name given to the Neolithic culture on the shore of the Western Black Sea. It is the source, presumably, that provided the domesticated animals that eventually became herds of cattle, sheep and goats herded on the steppes east of the Tripolye farmers by pastoralists, who – at first – were confined to the lower stretches of the river valleys of the Dnieper, Donets, Don, Volga, and Ural rivers. Surrounding these riverine communities, separated from each other by stretches of steppe, were undomesticated animals, mainly wild horses and gazelles, grazing on the abundant grasses. The herders living off the milk and meat of the herded animals thrived in and near the river valleys, where they also exploited the resources of fish in the rivers, as fish bones in middens attest. Being familiar with handling animals encouraged these pastoralists to domesticate the local wild horses, and when riding was innovated, the horse, previously used only for meat, became an aid in herding other animals, allowing larger herds, and moving out beyond the river valleys. This steppe herding society took shape c. 5000 BCE, and Anthony recognizes it as the population in which PIE originated and he sees the boundary between the steppe herders and the Tripolye farmers as a *'robust' material-culture frontier'* that persisted for centuries, which is his clue to seeing a linguistic boundary.

The southeastern European Neolithic was at first surrounded on all sides by 'foragers' as Anthony labels them, an appropriate single word for the usual phrase 'hunters and gatherers' especially as it includes 'fishers'. This persistent material culture frontier between the southeastern European farmers and foragers continued to be a robust frontier separating the herders who appeared on the steppe from the older culture of Tripolye farmers, but are we to accept that the language of the new herders was essentially unrelated to that of the earlier farmers?

If the herders obtained their domesticated animals from the farmers, wouldn't there have been some relationship? Since the nineteenth century a prominent explanation for the emergence of early pastoral societies was that boys and young men seasonally took animals from the villages to forage in the nearby grasslands that they could not cultivate, and when it became clear that subsistence was possible away from the farms, some of these men, probably motivated by pressures in the farming region (and women who chose to go with them) separated from their farming relatives and become mainly dependent on animal husbandry in the drier ecological environment. The herders' speech would have a level of relationship since they had diverged from the same original language. However, **if** foragers stole (or bartered) cattle and ovids from the Neolithic people (and didn't eat them all), they might have learned how to manage the animals they acquired; this latter possibility seems to be the process Anthony favors (p.119)

The 'horse, wheel – language' connection of the book's title is predicated on the claim that the horse, whose native habitat was the Eurasian steppes, was domesticated by speakers of an early Indo-European language, and further that the light spoked wheel, as opposed to the heavy solid wooden wheel, was invented by this same people. The light weight wheel with spokes made possible the creation of chariots, which when drawn by running horses, was the fastest vehicle known in antiquity. It ultimately became a deadly 'war wagon'. The association of chariots with Indo-European speakers was established by linguists who reconstructed the vocabulary for the chariot parts, including the spokes and axles of the wheels, and some modern historians of ancient societies recognized the ownership of chariots among IE peoples earlier than among other peoples. But the clincher was excavating evidence of the earliest spoked vehicles in the steppes east of the Ural Mountains in the approximate center of the Eurasian grasslands, and Anthony was in the excavation team that unearthed this spoked vehicle evidence. He suggests that inspiration for inventing wheels with spokes and axles may have come from solid wheeled wagons made by the Maikop culture, on the northern slopes of the Caucasus Mountains adjacent to the steppe.

"We will proceed with the assumption that Proto-Indo-European probably was spoken north of the Black and Caspian Seas, the Pontic-Caspian steppes, broadly between 4500 and 2500 BCE" (p.132). Presumably we are to see them as developing out of the Eurasiatic phylum of languages as defined by Joseph Greenberg, or the Nostratic phylum defined originally by Russian linguists, but Anthony doesn't venture into this level of discussion; he does however hold that the horsed herders were linguistically distinct from the agricultural peoples in the "old Europe" of southeastern Europe, which the late Marija Gimbutas made famous.

Between 4200 and 4000 BCE the PIE herders "spread into the lower Danube". This move "likely represented the initial expansion of archaic Proto-Indo-European speakers..." (p.133) These were the ancestors of the Hittites and related 'Anatolian' speakers. This was the first example of herders establishing themselves in control over farming populations, who he argues gave up their ancestral language and adopted IE. Anthony notes that England did not become Norman French speaking; he might have added that Bulgarians today still speak a Slavic language despite having been conquered and ruled for some time by Turkic-speaking Bulgars. Why then did the non-IE speaking farmers take over the language of the invading horse riding rulers? Appointing surrogates among the invaded populations to be responsible for order in their locality encouraged these individuals to learn the intrusive language (this was the recruitment of the local elite), and these speakers of a newly acquired IE ultimately encouraged others around them to do likewise. Anthony cites an anthropological example where this process worked. And of course there is the successful instance of Latin being adopted by Celtic speakers in what is now France and Spain.

Other expansions of IE groups, who were becoming dialectally different, spread in other directions: eastward as far as the borders of ancient China (Tocharian); southward toward and ultimately into India (Iranian/Indic), but not northward where the forested region was not accommodating for herding. The westward expansion from the Ukrainian and lower Danubian regions spread in a series of movements pushing Germanic into northern Europe via the Polish plains and Celtic and Italic up the Danube valley; while Greek, Illyrian and Phrygian went south, the first two into the Balkans and the latter into Asia Minor, but Anthony does not explain how Armenian, which some say is close to Greek, ended up going south via a route east of the Black Sea.

In 2100-2000 BCE there was large scale production of copper in mines in the Indo-European territory (p.435). Bronze Age technology made possible the chariot, and it was 2100 BCE that saw the development of the chariot in the southern Ural steppes (p.462); the early diffusion of the chariot began by 1800 BCE (p.411).

Anthony's work is impressive for the summation of his own series of researches and for setting them in the context of the relevant achievements of generations of scholars. He learned Russian and examined publications of Soviet and post-Soviet archaeologists, and is able to mediate between Western and Russian scholars who have in some fields different schools of scholarship. Also he obtained permission to excavate in Russian territory, in the steppes; his own work and that of some of his colleagues, including his wife, Dorcas Brown, form the centerpiece around which he draws in the information and interpretations of hundreds of earlier and contemporary contributors to the quest to flesh out the basic story.

"The societies that probably spoke classic Proto-Indo-European – the herders of the Yamnaya archaeological culture in the Pontic-Caspian steppes – were the first people to create a herding economy that required regular seasonal movements to new pastures throughout the year. Wagons pulled by cattle allowed them to carry tents, water, and food into the deep steppes, far away from the river valleys, and horseback riding enabled them to scout rapidly and over long distances and to breed on a large scale, necessities in such an economy." (p.133). Horseback riding in Anthony's assessment, began "probably before 4200 BCE" (*loc.cit.*). Earlier writers claimed riding was later, long after using horses for draft of carts and chariots, but Anthony's own research on the abrasion on the teeth of horses in excavated contexts proved that bits had been used to control horses when ridden by persons in the "archaic steppe herding societies" on the Pontic-Caspian steppes. How to build wagons, Anthony suggests, was learned by nearby PIE herders from the Maikop archaeological culture on the northern slope of the Caucasus Mountains, and this craft spread throughout the steppes. Interestingly, this postulates contributions to PIE material-culture of cattle from Tripolye, over the western Black Sea, and wagons from Maikop above the eastern Black Sea (cultures that apparently had no contact with each other), that would be possible in a long gestation for the emergence of PIE material-culture because PIE herders occupied the territory between Tripolye and Maikop.

One can agree that Anthony had bitten off enough to chew, and he didn't need to investigate a much newer science, population genetics, but he wasn't required to disparage that field. He asserts "Languages and genes are correlated only in exceptional circumstances, usually at clear geographic barriers, such as significant mountain ranges or sea – and often not even there. Anyone who *assumes* a simple connection between language and genes, without citing geographic isolation or other special circumstances, is wrong at the outset." That is pretty strong, but one might recommend that at some moment of leisure, if he ever has such, he peruse the magnum opus of L.L. Cavalli-Sforza, et al, The History and Geography of Human Genes (Princeton University Press, 1994). Unfortunately, the Eurasian steppes, the major foci for Anthony's work, had at the time provided few samples of genes for analysis, but the coverage of the world in so far as evidence was available is impressively presented. For example, Figure 5.5.3 "Zones of sharp genetic change in Europe and their correspondence with linguistic boundaries" (in HGHG, p.271) is interesting in comparing what Anthony writes about the spread of several branches of Indo-European into Europe. This tome of text and maps of genetic distances is essentially the summary of the early period (since c. 1950) of population genetics research; recent research, scattered in journals and a few books, includes a greater number of genes and alleles and a greater emphasis of mtDNA and Y-chromosome data, so that ultimately there will be more adequate quantities of relevant specific evidence on the PIE problem.

Anthony is not completely unaware of genetic evidence. "Language is strongly associated with persistent material-culture frontiers that are defined by bundles of opposed customs, what I will call *robust* frontiers," he declares (p.105). Among a number of possible examples, he chooses the Welsh / English frontier, and adds: "For many centuries men rarely married across this border, maintaining a genetic difference between modern Welsh and English men (but not women) in traits located on the male Y-chromosome." This statement makes one wonder why he made the statement discounting genetic evidence; it shows he has some familiarity with the population genetics literature.

Any brief review of a book as richly textured as this one is necessarily inadequate; Anthony lucidly surveys the many details that remain unexamined here, but this reviewer acknowledges that he acquits himself well – even as an "outsider" to linguistics.

The three nouns in the title – horse, wheel, and language – are reversed in their treatment. Language comes first, not only because it is the crux of the historical development of the research, but because it is the context for the archaeological data that Anthony marshals in his presentation.

Here is a question: what kind of a book was Princeton University Press intending to publish? Was it a volume for the general public or for a professional archaeological readership? It could be the latter, and in that case suitable to entice linguistically-skeptical archaeologists to consider a case for dovetailing the PIE data into an archaeological context, and that is a worthwhile project. But if intended for the general public, wherein there is an appetite for scientific explanations, then it seems to this reviewer that the format is wrong: much of the detailing and illustration of pot types from various excavations and the deductions based on them should have been published in archaeological journals and this book could have summarized the conclusions in a briefer and more easily comprehended format.

A word about the author's focus. He mentions that chariots were spread from Ireland to China, but doesn't mention the controversy over whether the Chinese chariots were borrowed from the steppe charioteers or independently invented in China – he merely implies that when he notes that the Shang kings of China, like the Mycenaeans in Greece both c.1500 BCE owed a common technology debt to the Late Bronze Age herders of Eurasia (p. 437). Some Chinese scholars maintain that China owes nothing to outsiders in the development of their culture; rather it is the other way around. Elsewhere Anthony skirts the controversy of Gimbutas's construct of a Goddess worshiping culture in "Old Europe". In both cases it was probably wise –though each dispute deserves attention –not to be distracted from his main focus. Why, he seems to think, unnecessarily get into turbulent waters?

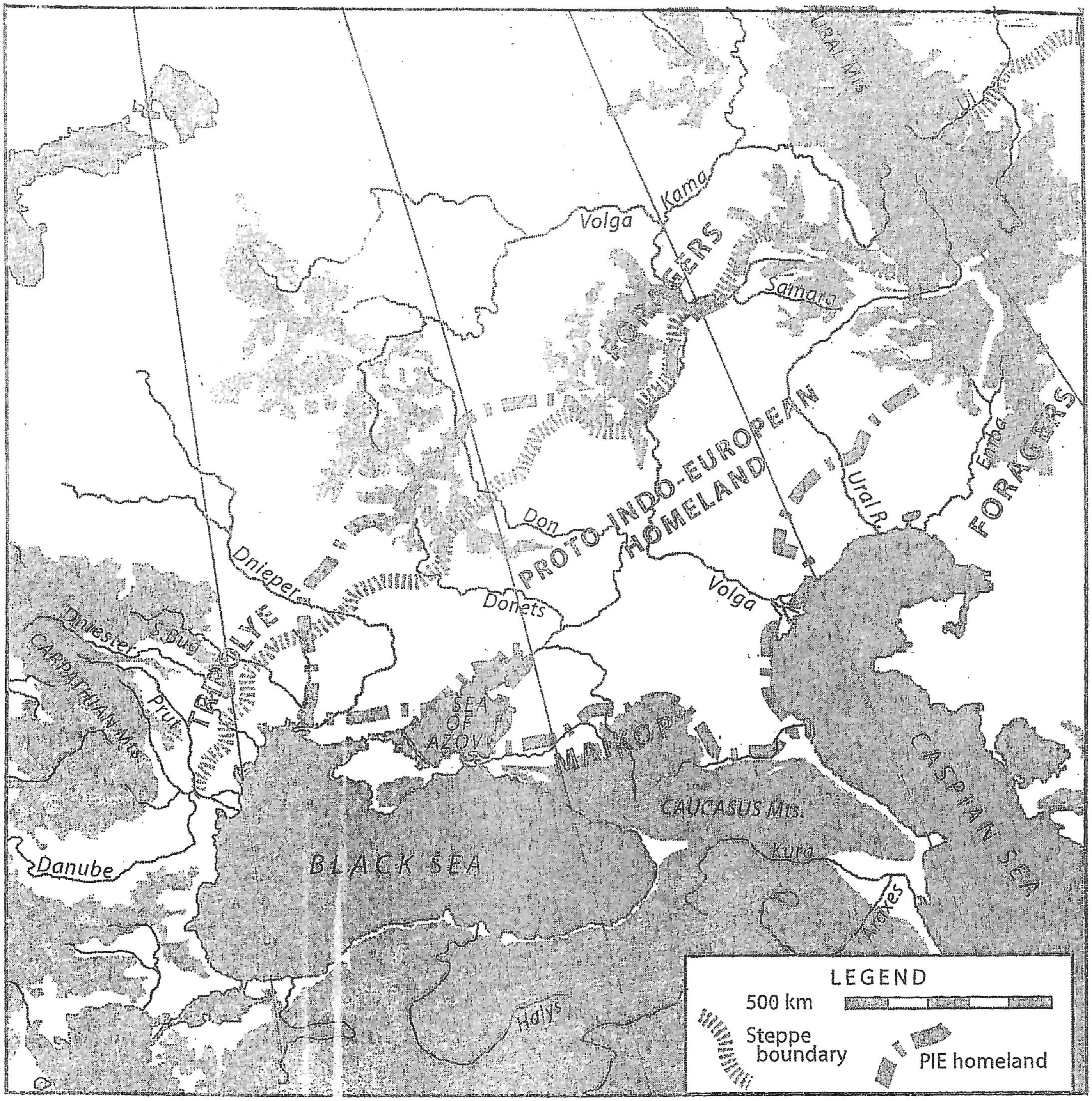
Ural Mts
Ui
Kama
Volga
FORAGERS
Samara
PROTO-INDO-EUROPEAN HOMELAND
Emba
Ural R.
FORAGERS
Don
Dnieper
Donets
Volga
TRIPOLYE
Dniester
S. Bug
CARPATHIAN Mts
Prut
SEA OF AZOV
MAIKOP
CAUCASUS Mts
Kura
Araxes
CASPIAN SEA
BLACK SEA
Danube
Halys
LEGEND
500 km
Steppe boundary
PIE homeland

Figure 5.1 The Proto-Indo-European homeland between about 3500–3000 BCE.

The Horse, the Wheel, and Language: How Bronze Age Riders from the Eurasian Steppes Shaped the Modern World, by David W. Anthony. Princeton University Press. 2007. 533 pages, maps, photos, drawings, tables, notes, appendix, bibliography, index.

Reviewed by Harold C. Fleming
Gloucester, Massachusetts

David Anthony is an archeologist, presently professor of anthropology at Hartwick College in Oneonta, New York. His wife, Dorcas Brown, is also an archeologist and field worker whose collaboration with David has been significant and long lasting. In a sense this is their book. David has been working on this topic and on this book for at least 20 years that I know of. Were he an older man this could be considered to be his Meisterwerk. It fully deserves to be called that and to be treated with the respect that such a label commands. Meisterwerks differ in the degrees to which they approach perfection. Tremendously influential or massively incorporating the wisdom of a field, such works as Darwin's *On the Origin of Species* or Lyell's *Principles of Geology* or Linnaeus' *Systema naturae* were accused of imperfections by some. So it is going to be with *The Horse, the Wheel and Language,* herein after HW&L. I will judge that, while being a major contribution to Indo-European prehistory and a model for Long Ranger projects, HW&L does fall short of perfection. As we will see the faults lie not in his own or Dorcas Brown's scholarship but rather in the scholarship of those he reveres.

HW&L is preoccupied with the homeland (UrHeimat) of a particular linguistic taxon, with the chronology of that taxon, and with evolution or development (Entwicklung) of that family of languages over time. The taxon in question is Indo-European (Indo-Germanic) or IE whose ancestral form is proto-Indo-European or PIE. But Anthony explicitly discusses a pre-proto-Indo-European or PPIE, as well as the not-quite-certain status of the Anatolian branch of the family (Hittite, Lydian, et al) which probably makes an Indo-Hittite stage slightly prior to PIE

Normally, a Long Ranger or an anthropological prehistorian would consult the data and methods characteristic of four fields, to wit, genetic linguistics, archeology, biogenetics, and physical anthropology. If possible, always, the field of ethnology might be involved, as well as history (documentary), botany, zoology (systematics) and geology. HW&L stresses two of these, archeology, especially field work, and genetic linguistics. Biogenetics is barely consulted and not without a smidgen of disdain. Fossil humans are examined extensively but taxonomy or "racial classification" is not handled well. Botany and zoology are crucial to many of the book's conclusions and their aid is used frequently and with great skill. But since modern archeological field reports stress floral and faunal aspects of any site, the involvements of botany and zoology are really part of archeology. Documentary history and ethnology are consulted frequently, especially to discuss contact with the old civilizations of the Near East and to bring the rich written traditions of India (Rig Vedas) and Persia (Avesta) to bear on cultural reconstructions of PIE.

One additional and salutary aspect of HW&L is the very strong involvement of Russian archeologists in the field work – after all most of it took place in their own country – but also in analysis and hypothesis formation. We have always stressed the international side of our work; indeed ASLIP was born in the cooperation of Russian and Euroamerican scholars. Thus the highest praise is due to David Anthony for learning Russian, doing field work in or near Russia, and carefully presenting a ton of field work done by Russian colleagues, primarily archeologists. I would bet that this book is being read by a host of Russian scholars and probably with great pleasure!

Before we examine the book in more detail we may address one general topic because it virtually never shows up in the details of HW&L. That is the role of biogenetics in a book of such a scale and importance. First question is: should biogenetic evidence be ignored? Probably not. Since it establishes or tends to establish genetic connections among populations, its relevance to any prehistory involving multiple population movements and contacts seems beyond question. However, a second question might be: is biogenetic evidence inevitably useful and understandable? Or, third, is the way geneticists present their conclusions applicable to someone's investigation? Or the fourth question which is being asked nowadays, given the flood of biogenetic articles and the confidence with which geneticists state their conclusions, is: how much bearing does a biogenetic conclusion have on a linguistic problem.

Impressionistically I might say, even without reading the whole literature, that geneticists tend to assume that language and genes are closely related. More concretely, for example, we might say that language A must be related to language B or used to be related to language B because their populations have close ties in biogenetic terms (Rhesus, HLA, Mitochondrial DNA, Y-Chromosome, etc.). So my answer to questions 2 and 3 would be NO, not always. But the 4th question gets a different answer – *it depends*. It depends on how the geneticists interpret their data and conclusions. And their chronological conclusions are rarely up to archeological standards. Or their conclusions are not appropriate to the prehistory involved but discover something else of interest. One example would be an article which traced some DNA across North Africa from Egypt, concluding that it must have come from Arabia and implying that Afroasiatic also came from Arabia. While their linguistic conclusion was ridiculous, their discovery may well have been the old DNA trail left by Neolithic farmers from Egypt or the Levant and datable to maybe 4500 BC.

Our general conclusion would probably be the one reached in the latter half of the 20th century – there is some correlation between "race" and language but it is so variable that it is basically an unknown. In some cases the correlation among race, language and culture is nearly ONE or 1.0 (100%) and in some cases it is 0.5 and in some cases practically zilch or 0.00 %.[1] It cannot be predicted and should not be relied on as a general correlation. Nevertheless it is clear that there is some correlation between language and genes in many cases. The conclusion is not that we should abandon all efforts to figure out how to work with genetic conclusions in prehistoric contexts but rather that we should try to find ways of working with it properly.

In the case of PIE one major drawback was probably the lack of DNA from the ancestral populations of the great Eurasian steppe. Since the HW&L team worked with an abundance of fossil humans from innumerable cemeteries, we have to ask why the author and his colleagues did not attempt to get some genetic information from them. After all fossils from, say, 6000 years ago probably would produce more useful DNA than fossils of, say, 30,000 years ago –which has been done on Cro-Magnon fossils. Just bring some of the tissues to our colleagues at Max Planck in Leipzig and see what they can find.

Anthony did use fossil materials, basically bones, in two other ways. On the one hand and very fruitfully he cites the presence or absence of dental caries as evidence for a diet of starchy grain foods versus a diet mostly of meat and /dairy products or fish. On the other hand he did a bit of race classification or population comparison on a few occasions, characterizing the people of Cucuteni-Tripolye as square-faced or rounder faced, as being "proto-Europoid" another time, and some steppe people as more narrow faced or Mediterranean. Yet another time another group of steppe people were seen as large or tall. Since the earlier literature and speculation about the Aryans or Indo-Europeans had frequently portrayed them as tall and blond, it would be pertinent to check this thesis against fossil evidence, especially when one had plenty of that. Moreover when Anthony determined that the "Old Europeans" of the Balkans spoke a language different from IE, one reasonable expectation would be that the two populations might be physically distinct.

Although the dying term "race" would be apt for discussing these population differences, let us use the old term "anthropometrics" even if its use is declining in biological anthropology. Anthropometrically we could have been given statistical comparisons with potential usefulness. That HW&L does not do that is unfortunate but understandable – the book did a lot of other things extremely well!

Before examining HW&L in more detail, it is useful at this point to rough out the main conclusions of the book, so as to set the stage by indicating the main thrust.

PIE: its daughters, their inventions and their adventures

That might suffice for a brief summing up of HW&L's main conclusions and arguments.[2] Despite a ton of archeological data and conclusions which form this book's core, it is focused strongly on **a linguistic** phenomenon – the well-known Indo-European family of languages. Unlike much contemporary research aimed at finding what larger linguistic class IE belongs to (e.g., Eurasiatic, Nostratic, Borean, etc.), HW&L is solely interested in the location of the homeland (UrHeimat) of PIE and its dates and correlating those with archeological cultures which emerge from the same geographical area, *viz.*, the great steppes of European Russia and Ukraine, and later extensions to the Altai. Since PIE and its daughters are associated with some famous and fruitful innovations in human prehistory, considerable attention is given to the inventions of the wheel and the chariot, horse riding and nomadic herding, and sundry lesser things.

Nevertheless the overall conclusions in linguistic and cultural terms are not especially new. Most of HW&L's conclusions I first heard as hypotheses in the 1950s in Yale's graduate program in anthropology. Since these were mostly associated with Marija Gimbutas, the present book might be called **Gimbutas on steroids.** Her theses have been vastly strengthened and spelled out and extended, while the startling hypotheses of Laird Renfrew have been dispatched to a recovery program.[3] Anthony seriously doubts that Anatolia is PIE's homeland; nor does he believe that "Old Europe" spoke IE.[4] And while he does not say very explicitly that he has confirmed an older hypothesis, that is what he has done! Granted that the Old Europe hypothesis had gotten somewhat romanticized, what with the Mother Goddess cults and all, still it is illuminated by contrast with the very carefully honed picture of the Indo-Europeans who were most assuredly not Old Europeans. And for this David Anthony should be given extra credit; he has given us a brighter picture of the old Neolithic people of southeastern Europe. Now freed from the burden of being the Aryans they may be examined more closely for their affinities elsewhere. Moreover their initial demise circa 4200-3800 BC due to a climate change to frigid conditions which injured their grain-based economy bids us to question whether their final demise was due to the heroic warriors of IE after all. Again we can thank David Anthony for bringing out this aspect of those times. This may give the climate determinists an extra arrow for their quiver. Having Labrador's climate would do Kansas little good!

Most books do not have such good summaries of their contents in their Tables of Contents as this one does. It is enough now to give said Table because it informs us marvelously about the details of the book. Forthwith the Table of Contents:

PART ONE: LANGUAGE AND ARCHAEOLOGY. 120 pages. [5]

Chapter One. The Promise and Politics of the Mother Tongue. 18 pages.
- Ancestors
- Linguists and Chauvinists
- The Lure of the Mother Tongue
- A New Solution for an Old Problem
- Language Extinction and Thought

Chapter Two. How to Reconstruct a Dead Language. 18 pages
- Language Change and Time
- Phonology: How to Reconstruct a Dead Sound
- The Lexicon: How to Reconstruct Dead Meanings
- Syntax and Morphology: The Shape of a Dead Language
- Conclusion: Raising a Language from the Dead

Chapter Three. Language and Time 1. The Last Speakers of Proto-Indo-European. 20 pages
The Size of the Chronology Window: How Long Do Languages Last?
The Terminal Date for Proto-Indo-European: The Mother Becomes Her Daughters
The Oldest and Strangest Daughter (or Cousin?): Anatolian
The Next Oldest Inscriptions: Greek and Old Indic
Counting the Relatives: How Many in 1500 BCE?

Chapter Four. Language and Time 2. Wool, Wheels, and Proto-Indo-European 24 pages
The Wool Vocabulary
The Wheel Vocabulary
When Was the Wheel Invented?
The Significance of the Wheel
Wagons and the Anatolian Homeland Hypothesis
The Birth and Death of Proto-Indo-European

Chapter Five. Language and Place. The Location of the Proto-Indo-European Homeland. 19 pages
Problems with the Concept of "the Homeland"
Finding the Homeland: Ecology and Environment
Finding the Homeland: The Economic and Social Setting
Finding the Homeland: Uralic and Caucasian Connections
The Location of the Proto-Indo-European Homeland

Chapter Six. The Archaeology of Language. 19 pages
Persistent Frontiers
Migration as a Cause of Persistent Material-Culture Frontiers
Ecological Frontiers: Different Ways of Making a Living
Small-scale Migrations, Elite Recruitment, and Language Shift

PART TWO: THE OPENING OF THE EURASIAN STEPPES. 345 pages

Chapter Seven. How to Reconstruct a Dead Culture. 9 pages
The Three Ages in the Pontic-Caspian Steppes
Dating and the Radiocarbon Revolution
What Did They Eat
Archaeological Cultures and Living Cultures
The Big Questions Ahead

Chapter Eight. First Farmers and Herders: The Pontic-Caspian Neolithic 26 pages[6]
Domesticated Animals and Pontic-Caspian Ecology
The Farmer-Forager Frontier in the Pontic-Caspian Region
Farmer Meets Forager: The Bug-Dniester Culture

Beyond the Frontiers: Pontic-Caspian Foragers before Cattle Arrived
The Gods Give Cattle

Chapter Nine. Cows, Copper, and Chiefs. 33 pages
The Early Copper Age in Old Europe
The Cucuteni-Tripolye Culture
The Dnieper-Donets Culture
The Khvalynsk Culture on the Volga
Nalchik and North Caucasian Cultures
The Lower Don and North Caspian Steppes
The Forest Frontier: The Samara Culture
Cows, Social Power, and the Emergence of Tribes

Chapter Ten. The Domestication of the Horse and the Origins of Riding: The Tale of the Teeth. 32 pages
Where Were Horses First Domesticated?
Why Were Horses Domesticated?
What is a Domesticated Horse?
Bit Wear and Horseback Riding
Indo-European Migrations and Bit Wear at Dereivka
Botai and Eneolithic Horseback Riding
The Origin of Horseback Riding
The Economic and Military Effects of Horseback Riding

Chapter Eleven. The End of Old Europe and the Rise of the Steppes. 38 pages
Warfare and Alliance: The Cucuteni-Tripolye Culture and the Steppes
The Sredni Stog Culture: Horses and Rituals from the East
Migrations into the Danube Valley
The Suvorovo-Novodanilovka Complex
Warfare, Climate Change, and Language Shift in the Lower Danube Valley
After the Collapse

Chapter Twelve. Seeds of Change on the Steppe Borders: Maikop Chiefs and Tripolye Towns. 37 pages
The Five Cultures of the Final Eneolithic in the Steppes
Crisis and Change on the Tripolye Frontier: Towns Bigger than Cities
The First Cities and Their Connections to the Steppes
The North Caucasus Piedmont: Eneolithic Farmers before Maikop
The Maikop Culture
Maikop-Novosvobodnaya in the Steppes: Contacts with the North
Proto-Indo-European as a Regional Language in a Changing World

Chapter Thirteen. Wagon Dwellers of the Steppes: The Speakers of Proto-Indo-European. 40 pages
Why Not a Kurgan Culture?

  - Beyond the Eastern Frontier: The Afanasievo Migration to the Altai
  - Wagon Graves in the Steppes
  - Where Did the Yamnaya Horizon Begin?
  - When Did the Yamnaya Horizon Begin?
  - Were the Yamnaya People Nomads?
  - Yamnaya Social Organization
  - The Stone Stelae of the North Pontic Steppes

- Chapter Fourteen. The Western Indo-European Languages. 31 pages
  - The End of the Cucuteni-Tripolye Culture and the Roots of the Western Branches
  - Steppe Overlords and Tripolye Clients: The Usatovo Culture
  - The Yamnaya Migrations up the Danube Valley
  - Yamnaya Contacts with the Corded Ware Horizon
  - The Origin of Greek
  - Conclusion: The Early Western Indo-European Languages Disperse

- Chapter Fifteen. Chariot Warriors of the Northern Steppes. 41 pages
  - The End of the Forest Frontier: Corded Ware Herders in the Forest
  - Pre-Sintashta Cultures of the Eastern Steppes
  - The Origin of the Sintashta culture
  - Warfare in the Sintashta Culture: Fortifications and Weapons
  - Tournaments of Value
  - Sintashta and the Origin of the Aryans

- Chapter Sixteen. The Opening of the Eurasian Steppes. 46 pages
  - Bronze Age Empires and the horse trade
  - The Bactria-Margiana Archaeological Complex
  - The Opening of the Eurasian Steppes
  - The Srubnaya Culture: Herding and Gathering in the Western Steppes
  - East of the Urals, Phase I: The Petrovka Culture
  - The Seima-Turbino Horizon in the Forest-Steppe Zone
  - East of the Urals, Phase II: The Andronovo Horizon
  - Proto-Vedic Cultures in the Central Asian Contact Zone
  - The Steppes Become a Bridge across Eurasia

- Chapter Seventeen. Words and Deeds. 10 pages
  - The Horse and the Wheel
  - Archaeology and Language

Appendix: Author's Note on Radiocarbon Dates. P.467
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General Comments

HW&L is a tremendous book. It should fairly well settle the questions about IE origins which are still debated by some scholars. It should write a FINIS to efforts by scholars like Alinei to drag IE deep into the Paleolithic or the jingoistic scholars of India who seek to derive IE from South Asia or to the group striving to correlate all or most linguistic expansions with the agricultural Neolithic.

But it is not the linguistic portion of HW&L which is overwhelming; it is the rock solid mass of archeological evidence which wins the day. Our understanding of IE and its mother, PIE, was already in great shape before this book was published. Indeed the study of IE is the finest accomplishment of historical linguistics, at least in the sphere of reconstruction. In taxonomy the classifications of Joseph Greenberg in Africa, the Americas, and the southwestern Pacific reign supreme. Taxonomy is definitely not the forté of Indo-European studies or Indo-Europeanists, given the general reluctance of most of them to even consider linking IE to Uralic or Altaic when that relationship is very nearly obvious. Johanna Nichols, one of Anthony's gurus, is even quoted as calling such a relationship a "quasi stock". I would bet Carl von Linné would be shocked to hear of such low level caution in taxonomy. He might be inclined to join Robby Burns in depicting that as a "wee sleekit cowring timorous beastie". But really the question is –is there any other science in which people are so inordinately proud of such anxious little acts of classification, yet outraged by bolder colleagues? Is the whole field of contemporary short range linguistics to be portrayed as a collective anxiety neurosis? Can this mental derangement be cured?

There are some elements of advice to give David Anthony. As some of our old hands may recall, David was with us in ASLIP for some years, during which we featured some of his work and that of James Mallory of Belfast. In HW&L David has abandoned any piece of cognition which smacks of Long Range Comparisons. His advisers are almost all from the University of Pennsylvania (which has produced such famous linguists as Zelig Harris and Noam Chomsky) but David's advisers are all very conservative Indo-Europeanists, including Johanna Nichols[7] whose global efforts (or "typology run amuck") seek systematically to remove Greenbergian taxonomies from said globe.

So our advice to David Anthony would be first of all to add some Russian linguists, Long Range type, to his roster of Russian colleagues. He may learn how good Indo-Europeanists can ever venture into Nostratic studies or Dene-Caucasic.[8] Secondly, we might suggest that he can learn something about homelands and linguistic dating from the literature or from long rangers, even if his gurus swear that their methods are the only ones which work.

It has to be said that the IE methods which David used to locate the PIE homeland worked excellently. Using the reconstructed list of flora and fauna plus lexical exchanges with neighboring families, one can compile a set of criteria for ascertaining the homeland. What sort of place is implied by proto-words for various features of the environment and the plant life and the animals? Dating PIE was aided greatly by a string of old written IE languages, such as Hittite, Lydian, Luvian, Lycian, Avestan Iranian, Old Indic, Mycenaean Greek, Phrygian, Thracian, Dacian, Latin., Oscan-Umbrian, Gothic, Old Norse and many later offspring of many of them. Merging these varieties with historical dates associated with them plus the archeologically attested material culture to be associated with them, one solves a crossword puzzle type problem to arrive at a most likely time span in which to place the proto-language.

The trouble with these methods, and yes despite the crowing about their superiority as part of the Comparative Method, is that they do not work very well in most other regions of our planet. Obviously, the methods demand a high level of accurately reconstructed lexicon and a great deal of good archeological work relevant to the time period involved. This takes a lot of good descriptive linguistic work over several generations and it works best if the languages are not so remote from each other descriptively or chronologically. I believe that more linguists work on, and for generations have worked on, the 150 or so IE languages than have worked on all the other 4825 languages of the world put together.[9] Probably only Semitic, Sinitic, and Japanese can come even close to the IE work force.

This still cannot work very well without a lot of good archeological work that is dated well. What good would it do to propose that proto-X was probably located in Area-Y at Time-Z if nobody knows anything about that locale at that date – archeologically. This has been the situation with many of my own hypotheses in Ethiopia and the Horn – there is far too little archeology where I propose ancestral languages. It becomes impossible to test hypotheses! Moreover the depth and frequency of archeological cultures in the IE regions renders HW&L's correlations of language group cum archeological culture **far more convincing** than the usual "by guess and by golly" correlations so often quoted in the literature of prehistory.[10]

What methods might we recommend to David Anthony when he moves to another region, one with fewer good reconstructions and less archeology? How can he generate hypotheses about homelands and their dates without the tried and true IE methods? Actually the science of historical linguistics generated two heuristic proposals a long time ago – glottochronology and distribution theory – both of which Anthony ignored totally. He knew about glottochronology because he talks about "the Swadesh list." We must infer that he is disdainful of that kind of linguistic dating.[11] Some kind of distribution theory has been around for a long time (*e.g.*, looking for the centers of diversity in a phylum, etc.) but Isidore Dyen formalized the heurism, stressing the importance of internal taxonomy in locating the likeliest original dispersal point for a group of languages.[12]

When glottochronology and distribution theory are applied to IE in order to locate and date PIE, they produce pretty much the same results as obtained by David Anthony using his pure IE methods. Since IE is more complicated than normal phyla because it has 10 to 12 separate and equal branches, I propose an easy exercise. First, look in the back of the *American Heritage Dictionary*, 3rd edition, 1996. Second, take note of the numerous branches, including the poorly known Thracian, Phrygian, and Illyrian. Third, plot each branch on a map of Eurasia. This is the hard part because you have to locate the center of each branch. Then fourth, solve the puzzle of the distribution by using the "principle of the least moves," *i.e.*, what location involves the fewest moves by each branch in joining the others in a single area? This will remind you of William of Occam's principle.

Since the taxonomy given by Calvert Watkins in the American Heritage Dictionary is not quite the same as that used by David Anthony, we immediately run into a serious problem. **Anatolian.** Watkins treats Anatolian as one of many branches, but Anthony vacillates between IE and IH (Indo-Hittite). If IH is the preferred classification, then Anatolian is a true coordinate to the rest of IE. Suddenly, in Dyen's terms, Anatolian becomes language A, while everybody else becomes language B. When it is said that Anatolian is the first to leave the homeland, it is equally logical to say that Anatolian was in the homeland while the other half (IE) left the homeland. Nothing in distribution theory can solve an A *vs*. B standoff; either A moved or B moved.[13]

There is another theoretical requirement, however, which may aptly be called 'common sense'. When, as often happens, two proposed homelands can be hypothesized for the same family, then a simple additional rule can be applied. If one proposed homeland is occupied by another family's known or proposed homeland, then abandon it and choose the other homeland. That seems to be the case with the IE versus IH problem. Not only can Anthony sketch out the trail followed by Hittite and her kin from the PIE homeland but also Anatolia was in all probability occupied by another phylum, **Caucasic,** with another phylum, **Kartvelian**, sharing eastern Anatolia.[14] Since these two phyla are only found in or near the Caucasus cum Anatolia, and two IE languages have Caucasic substrates, there is not much room for PIE in Anatolia! This important conclusion was pointed out to Renfrew several years ago by James Mallory in Philadelphia.[15]

The last thing to mention is that HW&L will be an invaluable reference book for any one interested in the archeology of southeastern Europe or southern Russia and the steppes, as well as the eternal problem of IE origins.

An interesting postscript to this review would be the striking parallelism between IE pastoralism and that of Africa, especially that of ancient Africa. When Anthony discusses the "herders and gatherers" of Yamnaya culture circa 3400-3200 BCE, especially eastern Yamnaya, he touches on the anomaly of "tall and robust" people with meat and dairy diet but no sign of domesticated grains in their diet. Yet their diet had a portion of vegetable food which was not raised in riverine plots or seeded in occasionally visited areas; it was **wild**. Specifically seed bearing plants **Chenopodium** and **Amaranthus** which bear as much food as wheat or barley per acre but which are more nutritious. Such a pattern is rare in the modern world among pastoralists of Africa or the Middle East, if not absent, but it was the dominant pattern of the first pastoralism on record, the Saharan Neolithic which had cattle, lacked sheep and goats, but had wild grains Millet and Sorghum which were eventually domesticated. The bulk of modern African farmers, outside of Ethiopia and North Africa, grow millets and sorghums as the predominant grains in their diet; they are also quite important in southwestern Ethiopia. Their domestication was clearly independent of the Levantine Neolithic. Cf. Barich 1998 (passim), Marshall and Hildebrant 2002, and Fleming 2006 (150-157).

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[1] The case of the Polar Eskimo is often cited as a 100% correlation, based on a famous doctoral examination at Columbia University when the candidate confounded his Boasian-inclined faculty committee with the obvious. Completely negative cases are rare or may not even exist. The epitome of a very low correlation would be a population of English-speaking former African slaves in the American south or Caribbean islands. Or the French-speaking Haitians. However some gene flow from the slave owning populations of Europe would be present in the former slaves.

[2] We might have said "her daughters" because of the long tradition of rendering 'language' in the feminine gender. But with a language group and culture so powerfully focused on patri- type things using the feminine seemed wholly inappropriate.

[3] Anthony lists some seven publications by Marija Gimbutas, the earliest of which is her 1956, *The Prehistory of Eastern Europe, Part 1.* Cambridge: American School of Prehistoric Research Bulletin 20. He cites eight publications of Colin Renfrew, the earliest relevant one of which is his 1987. *Archeology and Language: The Puzzle of Indo-European Origins.* London: Jonathon Cape.

[4] Anthony clearly stipulates a population of Hatti speakers underneath the Hittites at least, concluding also that the IE Hittites were always predominantly a managerial class while the general population was Hatti. But he seems unable to realize that the Hatti were almost certainly North Caucasic speakers. Such a conclusion would, of course, seriously undermine the notion that Anatolia was the PIE homeland.

[5] When are Americans going to give up that old British spelling?

[6] Note to Readers. Anthony has adopted a different meaning for "Neolithic" in this section, based on the usage of Russian archeologists. Oddly enough to a Westerner it does not necessarily include the meaning of farming or plant domestication. Later on Anthony uses "forager" as a cover term for hunter and gatherer.

[7] Nichols has been at U/California @ Berkeley. I don't know her relationship to U/Penn.

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[8] Many years ago one of them told me that the long ranger type Russian linguist was still very much a minority among Russian linguists who were most thoroughly committed to IE methods. He explained that he and his colleagues had to be very careful not to violate traditional methods because otherwise the conservative fellows would "slaughter" them. Perhaps we should add "social conformity" or Agoraphobia to the list of attributes of the collective anxiety neurosis?

[9] If the many distinctive "dialects" of Romance and Indic were counted , IE would probably total more like 175 languages. The total of 5000 languages is borrowed from Merritt Ruhlen's GUIDE. Other scholars count more, some reach 6000, and the differences probably do not matter.

[10] The more archeological cultures in a given time/space area the more difficult the final correlation. This means that the prehistorian has to define the reasons for making a particular correlation much more precisely. Having only two or three archeological cultures in a large time/space area makes the guessing much easier; one's chances of being right just fortuitously are much greater. And just for that reason the odds are greater that the correlation will turn out to be false or misleading. This has been our problem in Africa. There are scads of obsidian points, flakes, blades lying around on the ground in central Ethiopia but no one knows how old they are or from what cultures they come. That situation may be starting to change with new archeological activity.

[11] This is unusual for an archeologist. While the main mass of linguists indulged themselves in hysterical rejection of glottochronology and delighted in its shortcomings, quite a few archeologists tried it out during the 1950s and 1960s. Some found it useful.

[12] Perhaps the clearest use of Dyen's proposal can be seen in Austronesian where it was used to counter Dyen's own notion of where the homeland was (Melanesia). When Robert Blust's classification proposed that three of the four primary taxa (sub-phyla) of Austronesian were located on the island of Formosa, while all the other (nearly 1000) languages were spread from Madagascar to Hawaii, it became clear if not self-evident that Formosa was the most likely place to locate proto-Austronesian. The same sort of argument, only more powerfully attested, derives the huge Bantu distribution (500 languages in one third of Africa) from the Nigeria-Cameroon border area.. Did we not know that Arabic came from Arabia the facts of Semitic distribution would encourage us to propose Arabia as the dispersal point of Arabic.

[13] Watkins also mildly disagrees about the homeland and its date. He locates PIE as "somewhere between eastern Europe and the Aral Sea" and dates it to 5000 BC (once) and the 5th millennium BC (once). Perhaps we can say between 4000 and 5000 BC. Naturally A *vs.* B problems can be solved archeologically by indicating movements through time from some locus to another.

[14] Gamkrelidze and Ivanov seem to have confused Anthony about the relationship between Kartvelian and IE. The glottalic theory of PIE consonants does not necessitate a special contact between PIE and Kartvelian. When he talks about contact between the Maikop people and the IE people of the Pontic steppes, he really is talking about the Circassian division of (North) Caucasic. Some years ago John Colarusso proposed a number of cognates between PIE and (North) Caucasic. Anthony also associated Afroasiatic with the first cattle to reach PIE speakers; the word basically [twr] is peculiar to Semitic where it is an innovation. The first domesticated cattle belonged to the Saharan Neolithic. Either the African cattle themselves or the custom of domesticating wild cattle spread up the Nile Valley and crossed over into the Levant where they were, I believe, new to the Semitic Neolithic folk there.

[15] However it may be rebutted by a belief that, since Anatolia is a large place, western Anatolia was not occupied by Caucasic or Kartvelian. Note, however, the presence of a (North) Caucasic language in far western Turkey – the Pakhy language, a relative of Hatti and the Circassian (Kabardian) branch of Caucasic. Furthermore, the presence of Semitic loan words in PIE is taken by some as evidence of contact between Semitic and PIE in Anatolia. Others attribute the Semitic loan words to trade or cultural diffusion in and out of Anatolia. No doubt the earlier diffusion of Neolithic crops from a Semitic-speaking Levant to a non-Semitic Anatolia could have resulted in considerable linguistic and cultural borrowing by the native Anatolians from the Semites. I know of no one, other than Militariev who is cited by Anthony, who believes that the Semites occupied any significant portion of Anatolia. The other suggestion that Afroasiatic itself originated in the Levant enjoys virtually no support among Africanists. The non-Semitic herders of the Fertile Crescent who domesticated ovicaprids and interacted with the Semitic grain growers were themselves strong enough to resist Semitic migration. Those autochthonous herders are most likely to have been Sumerian speakers or Elamites or Caucasic speakers. Or perhaps Kartvelian. There is possibly an archeological answer to these questions. Even today and despite the success of Islam a frontier exists

between the Semitic Near East and the non-Semitic Near East – the Turkish border. This David Anthony would call a persistent frontier. It corresponds to the Taurus mountains.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory, Issue XII (2007)

Editorial: Linguistic Hypotheses and Their Development

John D. Bengtson
Association for the Study of Language in Prehistory (Vice President)

One of the late Sergei Starostin's major accomplishments was the reformulation and development of the Sino-Caucasian (*a.k.a.* Dene-Caucasian, or DC) hypothesis. I myself attempted to follow his lead and develop the hypothesis along certain lines (*e.g.*, the inclusion of Basque and Burushaski). As with any path-breaking theory, DC has been the subject of extensive criticism, which is basically a necessary and beneficial part of scientific discourse, when conducted, as much as humanly possible, in an objective manner.

Unfortunately, some critics have overstepped the bounds of civil discussion and made unsubstantiated claims that go far beyond the objective assessment of data. For example, the late Larry Trask (1997) ridiculed the Dene-Caucasian hypothesis as an "enthusiastic hoovering up of isolates," and Alexander Vovin (2002) calls DC an "imaginative but futile attempt of human mind" motivated by "a religious belief in macro-families" rather than by scientific rigor. These are serious charges because they go far beyond (or rather sidestep) discussion of data and methods and instead level *ad hominem* attacks on the proponents of a hypothesis, namely Starostin, and by extension me, and anyone else who finds macro-family hypotheses probable.

The statements by Trask (see also Trask 1994-95, 1995) and Vovin (see also Vovin 1997) make me wonder what would motivate a linguist to hold a "religious belief in macro-families," and then to go out and "enthusiastically" recruit languages to add to one's favorite "imaginary" macro-family! It seems patently silly to me, but apparently some "mainstream" historical linguists believe something like this. Let me suggest instead that the attitude that macro-families *do not* exist (in spite of massive evidence to the contrary) may itself be a "religious belief."

Here I quote a statement by Harold C. Fleming (1994), himself an adventurous mind and founder of the Association for the Study of Language in Prehistory:

> Our greatest strength, us long rangers, is hypothesis breeding or creation. Next to that is our better understanding of what science is all about. Historical linguistics is not a branch of mathematics or formal logic. As Greenberg [1995] has argued brilliantly ... the concept of proof is misused by linguists – from a scientific standpoint. ... Charles Darwin's theory has never been proven, has it? It just gets less and less likely to be false or more and more likely to be true. 'Proof' is for algebra or courts of justice.

As shown by Charles Peirce scientific hypotheses are created by abductive reasoning, the process of reasoning to the best explanation for a set of facts. Though abductive reasoning, like all modes of reasoning, is fallible, it remains the only logical process that can create new knowledge. Once the hypothesis has been formed, predictions may be inferred by deduction and tested by induction (Peirce 1878; Anttila 1972). This is the scientific process by which Darwin's theory (or any successful theory) "gets less and less likely to be false or more and more likely to be true." As put by Joseph H. Greenberg:

> In all empirical sciences ... all that we can get are results so close to certainty that for all practical purposes we can consider them true, that is, a hypothesis which is overwhelmingly better than any other in accounting for the facts. (Greenberg 1995: 207)

Another eminent linguist, ASLIP Council Fellow Sydney M. Lamb, puts it this way in an autobiographical sketch:

> The in-group ["mainstream" historical linguists] says you can't accept any proposal of a genetic relationship among languages unless it has been proved beyond a reasonable doubt; and this just about precludes accepting *any* proposal of a distant relationship. These are people, I guess, who don't take their umbrellas or rain coats to work when the weatherman says there is a 70% chance of rain; it would have to be 100%. I defended Swadesh and I now defend Greenberg because to me, perhaps under the influence of my knowledge of physical science, it makes more sense to operate at any time with the most likely working hypothesis, even though that hypothesis may turn out to need revising when further evidence comes in. If biologists and physicists held out for 100% proof before adopting a working hypothesis (in which case it would not be a working hypothesis after all, would it?), they would never have made the progress we have seen during the past two hundred years ...

I therefore suggest that we approach hypotheses such as Dene-Caucasian, Eurasiatic, Nostratic, Austric, Indo-Pacific, etc., not as theories to "prove" or "disprove," but as working hypotheses and explanatory models that are subject to constant modification and correction. Note that Trask and Vovin have not offered alternative models to the ones they claim to "destroy." They have merely dismissed entire hypotheses because they have found details that are incorrect, and instead of suggesting a revised model they throw everything out, and claim that macro-families are not possible to demonstrate.[1]

Greenberg, Starostin, and others have proposed macro-family hypotheses as models that aim to explain the dispersal of human beings and their languages and cultures throughout the world. This has nothing to do with "religious belief," any more than natural selection and continental drift – hypotheses that were, at first, met with violent opposition – do. Instead this is the Process of Science: the formulation of hypotheses through abductive reasoning, and the testing that either strengthens and confirms, or eliminates them.

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Bengtson, John D. forthcoming. "Language Families of Northern Eurasia: Inference to the Best Explanation." In *In Hot Pursuit of Language in Prehistory: Essays in Honor of Harold Crane Fleming.* Amsterdam: John Benjamins.

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[1] See the recent article by Dybo & [George] Starostin (2008), which brilliantly discusses these issues in detail. Bengtson (forthcoming) further develops the concept of "best explanation" and "most likely hypothesis."

Dybo, Anna V., & George S. Starostin. 2008. "In Defense of the Comparative Method, or the End of the Vovin Controversy." In: *Aspekty komparativistiki / Aspects of Comparative Linguistics,* v. 3, 119-258. (Orientalia et Classica: Papers of the Institute of Oriental and Classical Studies XIX.) Moscow: RSUH Publishers, 2008, pp. 109-258. http://starling.rinet.ru/Texts/compmeth.pdf

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Vovin, Alexander. 1997. "The Comparative Method and Ventures beyond Sino-Tibetan." *Journal of Chinese Linguistics* 25.2: 308-336.

Vovin, Alexander. 2002. "Building a *'Bum-pa* for Sino-Caucasian." *Journal of Chinese Linguistics* 30.1: 154-171.

Book Notices

Fleming, Harold C. 2006. *Ongota: A Decisive Language in African Prehistory.*

An international team rediscovered a tiny tribe of hunters, first discovered a century ago in extreme southern Ethiopia but never seen again. Now dying out, Ongotan culture and language are kept alive by 20 old men who resist the pressures of two outside societies. A short description of their language and ethnography (published elsewhere) are given more fully. The examination of Ongota reveals an Afrasian (Afro-Asiatic, Hamito-Semitic) language of marked dissimilarity to its sisters in grammar and a large lexicon with links to Afrasian languages spread over large sections of Africa. Ongota clearly is in a class by itself within Afrasian, even though loan words from nearby languages muddy up the analysis. Ongotan has serious implications for Afrasian prehistory as a whole and hence the prehistory of northern and eastern Africa. Traditionally, some scholars (especially geneticists) have assumed a constant flow of culture, language, and genes from the Near East to the west and south of Africa, especially the Sahara and the Horn. With the bulk of its deepest or oldest branches located in the Horn Afrasian must surely have expanded into the Near East from the Horn. Recent archaeology confirms this conclusion, as do palaeobotanical studies.

IX, 214 pages; cloth bound; publishing date: August 2006; 78,00 Eur[D]
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verlag@harrassowitz.de

* * *

Bomhard, Allan R. 2008. *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology, and Vocabulary.*

This monograph deals with the comparison of Proto-Indo-European with various languages and language families of northern Eurasia, the Iranian plateau, the Indian subcontinent, the Near East, and northern Africa to determine whether or not there is evidence for a genetic relationship. The author concludes that the evidence points strongly to a genetic relationship within the framework of a "Nostratic macrofamily." Emphasis is placed upon traditional methodologies such as the Comparative Method and Internal Reconstruction.

This book is the first to deal with all aspects (comparative phonology, morphology, and vocabulary) of the languages and language families under consideration. Previous investigations into the possibility that Proto-Indo-European might be related to other languages and language families concentrated primarily on comparative vocabulary.

Publisher: E. J. Brill; Year: 2008; Pages: 2 volumes 1,811 pages; List Price: $446.00.

* * *

The *Aspects of comparative linguistics* series:

For more information and to obtain copies contact George Starostin
gstarst@yandex.ru.

Аспекты компаративистики. 1.

Aspects of comparative linguistics. 1. Ed. by A. Dybo, G. Starostin. - Moscow: RSUH[1] Publ., 2005. - 504 pp. (Orientalia et Classica. VI); ISBN 5-7281-0660-9

Aspects of comparative linguistics is a collection of papers on diachronical linguistics, periodically published from now on by the Centre of Comparative Linguistics of the Institute of Oriental and Classical Studies of RSUH. The first issue has been prepared specially for the 50th anniversary of the Head of the Centre, Prof. S. A. Starostin, and is dedicated to problems actual for those fields of research which constitute S. A. Starostin's main specialities, such as long distance genetic relationship, Indo-European studies, computer methods in linguistics, and Sinology.

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[1] Russian State University for the Humanities / Российский государственный гуманитарный университет.
[2] All but three of the articles are in Russian.

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Aspects of comparative linguistics. 2. Ed. by A. Dybo, G. Starostin. Moscow: RSUH Publ., 2006. 500 pp. (Orientalia et Classica: Papers of the Institute of Oriental and Classical Studies. Issue VIII.)

The second issue of *Aspects of comparative linguistics* contains a number of presentations and articles from participants of two scientific conferences on comparative-historical linguistics, dedicated respectively to the memory of V. M. Illich-Svitych (October 16-18, 2004) and S. A. Starostin (March 24-27, 2006). Topics range from specific problems of modern day Indo-Europeanistics, Uralistics, etc., to broader problems of general paleolinguistics and macro-comparison.

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Moscow: RSUH Publ., 2007.— 736 pp.— (Orientalia et Classica:
Papers of the Institute of Oriental and Classical Studies; Issue XIX).

The third issue of *Aspects of comparative linguistics* is dedicated in its entirety to the memory of Sergei Starostin, founder of the Center of Comparative Linguistics of the Institute of Oriental and Classical Studies of RSUH. A whole series of articles deal with particular as well as methodological questions of long-range comparison, touching upon language families as diverse as Altaic, Nostratic, Sino-Caucasian, and Khoisan. The rest of the works, written by Sergei Starostin's colleagues and disciples, deals with various issues of Indo-European and Uralic studies, as well as computer linguistics and theoretical problems of historical linguistics.

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Notice to Readers

Books Available for Review: *The Journal of Indo-European Studies Monographs*

John H. Brown, Publications Assistant for the *Journal of Indo-European Studies*, has invited our readers to review books from their series of monographs. You can view the complete listing at their web site: www.jies.org. The list currently comprises 52 numbered monographs, plus two unnumbered book titles, all of which are all kept permanently in stock.

If you see any titles that interest you, please contact *Mother Tongue* Editor John D. Bengtson. (See inside front cover for contact information.) Please bear in mind that we are primarily interested in broader issues pertaining to the Indo-European family in prehistory (especially archaeology, cultural change, and population movements), and *not* "micro-issues" such as the development of a single word or phoneme. Also check with the Editor in case the book has already been reviewed in *MT*.

Following are some of the titles we would especially like to see reviewed. See www.jies.org for further details.

Perspectives on Indo-European Language, Culture and Religion
Volume 1: Studies in Honor of Edgar C. Polomé
Monograph No. 7 -- Edited by Roger Pearson

Perspectives on Indo-European Language, Culture and Religion
Volume 2: Studies in Honor of Edgar C. Polomé
Monograph No. 9 -- Edited by Roger Pearson

The Glottalic Theory: Survey and Synthesis
Monograph No. 10 -- By Joseph C. Salmons

The Anthropomorphic Stelae of the Ukraine:
The Early Iconography of the Indo-Europeans
Monograph No. 11 -- By D. Ya. Telegin & J. P. Mallory

Indo-European Religion after Dumézil
Monograph No. 16 -- Edited by Edgar C. Polomé

The Indo-Europeanization of Northern Europe
Monograph No. 17 -- Edited by Karlene Jones-Bley and Martin E. Huld

The Kurgan Culture and The Indo-Europeanization of Europe
Monograph No. 18 -- Papers by Marija Gimbutas
Edited by Miriam Robbins Dexter and Karlene Jones-Bley

Varia on the Indo-European Past:
Papers in Memory of Marija Gimbutas
Monograph No. 19 -- Edited by Miriam Robbins Dexter and Edgar C. Polomé

Language Change and Typological Variation.
In Honor of Winfred P. Lehmann on the Occasion of his 83rd Birthday
Volume 1: Language Change and Phonology
Monograph No. 30 -- Edited by Edgar C. Polomé & Carol F. Justus

Language Change and Typological Variation.
In Honor of Winfred P. Lehmann on the Occasion of his 83rd Birthday
Volume 2: Grammatical Universals & Typology
Monograph No. 31 -- Edited by Carol F. Justus & Edgar C. Polomé

Greater Anatolia and the Indo-Hittite Language Family: Papers presented
at a Colloquium hosted by the University of Richmond, March 18-19, 2000
Monograph No. 38 -- Edited by Robert Drews

Regional Specifics in Light of Global Models BC --
Complex Societies of Central Eurasia from the 3rd to the 1st Millennium.
Volume 1: Ethnos, Language, Culture; General Problems;Studying Statistics;
Studying Sintashta; The Eneolithic and Bronze Ages
Monograph 45 -- Edited by Karlene Jones-Bley and D. G. Zdanovich

Regional Specifics in Light of Global Models BC --
Complex Societies of Central Eurasia from the 3rd to the 1st Millennium.
Volume 2: The Iron Age; Archaeoecology, Geoarchaeology,
and Palaeogeography; Beyond Central Eurasia
Monograph 46 -- Edited by Karlene Jones-Bley & D. G. Zdanovich

Proto-Indo-European -- The Archaeology of a Linguistic Problem:
Studies in Honor of Marija Gimbutas
Edited by Susan Nacev Skomal and Edgar C. Polomé

Indo-European Origins: The Anthropological Evidence
By John V. Day